PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From May 1 to December 1, 1884.

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November 27, 1884.

December 1, 1884.

ANNIVERSARY MEETING.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

May 1, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Professor Luigi Cremona, of Rome, Foreign Member, was admitted into the Society.

In pursuance of the Statutes, the names of the Candidates recommended for election into the Society were read from the Chair, as follows:—

Allman, Professor George Johnston, LL.D.
Balfour, Prof. Isaac Bayley, D.Sc.
Baxendell, Joseph, F.R.A.S.
Bell, James, F.I.C.
Hartley, Professor Walter Noel, F.R.S.E.
Herschel, Professor Alexander Stewart, M.A.
Hudleston, Wilfrid H., M.A.
Lamb, Professor Horace, M.A.
McKendrick, Prof. John G., M.D.
Ransome, Arthur, M.D.
Roy, Prof. Charles Smart, M D.
Rücker, Professor Arthur William, M.A.
Thomson, Joseph John, M.A.
Warren, Colonel Sir Charles, C.M.G.
Watson, Professor Morrison, M.D.

The following Papers were read :—

I. "On the Cortical Connexions of the Optic Nerves." By D. J. HAMILTON, M.B., F.R.S.E., Professor of Pathological Anatomy (Sir Erasmus Wilson Chair) in Aberdeen University. Communicated by Professor J. S. BURDON SANDERSON, F.R.S. Received April 17, 1884.

(Abstract.)

1. The original statement made by Gratiolet that the optic tract is directly connected with every part of the cerebral hemisphere in man, from the frontal to the occipital region, is almost literally true.

2. The origins of the optics may be divided into two sets—ganglionic and cortical.

3. The fibres in the ganglionic set are derived from the corpora geniculata, pulvinar and corpora quadrigemina, probably also directly from the substance of the thalamus.

4. The cortical set join the chiasma and tract.

5. The junction of the chiasma with the cerebral cortex is brought about by means of "Meynert's commissure." The latter arises from the lenticular-nucleus-loop (Linsen-kern-schlinge), decussates in the lamina cinerea, and passes with the optic nerve of the opposite side. This commissure is connected to the cortex in the frontal region by the following means:—

It arises directly from the lenticular-nucleus-loop; the lenticular-nucleus-loop is formed by the junction, below the lenticular nucleus, of the striæ medullares; the striæ medullares form part of the fibres of the inner capsule, and the inner capsule is composed of the fibres descending from the cortex. I should think it very probable that the fibres constituting the striæ medullares come from the cortex of the same side.

6. The other cortical connexions of the optic join the tract as it winds round the pedunculus cerebri. They are the following:—

(*a.*) A large mass of fibres derived from the motor areas of the opposite cerebral hemisphere, crossing in the corporus callosum, entering the outer capsule, and joining the tract directly.

(*b.*) A great number of fibres uniting it to the temporo-sphenoidal lobe of the same side, and especially the first and second temporal convolutions.

(*c.*) A bundle of fibres, which spreads out into the gyrus hippocampi of the same side.

(*d.*) A large leash of fibres, which connects it directly with the tip of the occipital lobe forming part of the "optic radiation."

7. The tract is in all probability further connected to the cerebral cortex *indirectly*, that is to say, by the intervention of the ganglionic masses, in the following manner:—To the occipital lobe of the same side, through its connexion with the corpora geniculata and pulvinar, and the continuation of fibres from these backwards.

8. It is extremely doubtful whether the anterior corpus quadrigeminum is connected to the occipital lobe of the opposite side by a continuous band.

9. The "optic radiation" of Gratiolet going to the occipital lobe is composed of the following constituents:—

(*a.*) A branch to the optic tract directly.
(*b.*) " " corpus geniculatum internum.
(*c.*) " " " " externum.
(*d.*) " " pulvinar.

(*e.*) A branch to the substance of the thalmus.

(*f.*) The direct sensitive band joining the posterior third of the posterior limb of the inner capsule.

(*g.*) A large mass of fibres which runs between the Island of Reil and the tip of the occipital lobe.

10. In the evolution of the brain it seems very probable that the functions of the thalamus and corpora quadrigemina as optic centres are transferred in man and certain mammals in great part to the cerebral cortex.

11. The cortical fibres which I have described are probably not all connected with visual centres, but are one means by which motor and other centres of the cortex are educated, and their function elicited in response to visual stimuli.

II. "On the Connexion of the Himalaya Snowfall with Dry Winds and Seasons of Drought in India." By HENRY F. BLANFORD, F.R.S. Received April 14, 1884.

The present paper, as regards its subject-matter though not in form, is part of a general investigation of the rainfall of India, which has occupied much of my spare time for some years past, and the results of which are already partly embodied in a memoir which I hope, in the course of a few months, to issue as an official publication of the Indian Meteorological Office. The idea that the snowfall of the Himalaya exercises a direct and important influence on the dry land winds of North-Western India is not now put forward for the first time. It has been the subject of frequent reference in the annual reports on the meteorology of India since 1876, as well as elsewhere; and in a report on the administration of the India Meteorological Department lately issued, I summarised very briefly those points in the experience of the previous five years which have seemed to justify its provisional adoption as a basis for forecasting the probable character of the monsoon rains.

Relying on this experience, in the month of June last, I put forward in the Government Gazette, a note giving warning of the probability of a prolonged period of drought in the approaching monsoon season, and the result, if not in exact accordance with the terms of the forecast, has been so far confirmatory of the general idea, as to induce me to put the facts of past experience formally on record, and thereby challenge attention to the subject. If I am right in the inference that the varying extent and thickness of the Himalayan snows exercise a great and prolonged influence on the climatic conditions and weather of the plains of North-Western India,

it is probable, that with more or less modification according to the local geography, causes of a similar character will be found equally operative in other regions, and perhaps on an even more extensive scale.

Empirical Grounds of the Hypothesis.

The Year 1876 *and* 1877.—The probable dependence of dry land winds and of periods of drought on the extent of the Himalayan snowfields first occurred to me when inquiring into the meteorology of India in 1876, a year disastrously notorious for that failure of the rainfall which produced the last great famine in Madras and the Deccan; and the first reference to the subject, as far as I am aware, will be found in the official report for that year. In summing up the results of the detailed description of the meteorological features of the year, and in tracing out the apparent dependence of the drought on the remarkable and unseasonable persistence of dry north-west winds down the whole of Western India, I was led on to connect these successively with the high pressure prevailing in the Upper Provinces and Bombay, relatively to Orissa and Eastern India, and with the low temperature of North-Western India in the spring of the year, as contrasted with the abnormally high temperature of the east of the peninsula, and I remarked* that this latter fact seemed "to argue that some cooling influence more potent than usual was at work, probably in the Punjab and on the northern mountain zone, condensing the lower strata of the atmosphere and causing an unusual outflow of cooled air." A few lines further on, it was observed, "it has been shown in the foregoing description of the rainfall, that in the early months of the year, the rainfall was unusually copious over the North-Western Himalaya,† and the plains of the Punjab which border the foot of the mountains, and indeed, on the North-Western Himalaya the rainfall was above the average, more or less, throughout the greater part of the year. The rain on the outer hills is most frequently accompanied by snow on the main and higher ranges, and indeed, when I visited Rawalpindi in the early part of April, 1876, I was informed that snow lay in some thickness at Murree, a very unusual occurrence so late in the season, and the slopes of the Pir Punjal were visibly snowed down to a low level. The question is therefore naturally suggested, whether the great expanse of snow, presented under the circumstances by the southern slopes of the Himalaya, and much of which fell unusually late in the season, was not that cooling agent, to the existence of which the

* *Op. cit.*, page 96.

† As will be seen further on, this is rather overstated. The heavy unusual fall was restricted to March and April, and the total of the whole season was not above the average, except at Rawalpindi and one or two other stations.

winds, the temperature, and the pressure equally testify. That it was so seems not improbable."

The report containing these passages was written in the autumn of 1877, a second season of drought; but, in this year, most intense in the North-Western Provinces and the plateau of Rajputana and Central India, which provinces were swept persistently, up to the very close of the summer monsoon season, by parching land winds from the north-west and west. In the passage immediately following the above quotation, this fact was noticed in the following terms:—"In the early part of the present season (1877) the same phenomenon (an unusual snowfall) was repeated, and with even greater intensity, and this year the land winds have been so persistent in the upper provinces, and on the plateau south of the Ganges, as to cause an almost complete failure of the summer rains in that region; while again, those of Bengal have been up to the fall of an average season." And in a postscript to the report was given an extract from Colonel (now General) J. P. Walker's report on the operations of the Great Trigonometrical Survey, viz., a passage taken from the narrative report of Mr. E. C. Ryall, in charge of the Kumaon and Garhwal party of the Togographical Survey. It is as follows:—"The winter of 1876–1877 proved to be the severest known for many years past among these hills; the spring was wet and cold, and felt quite wintry. . . . . (The wet weather) continued with few intermissions till the 8th June, the most noticeable break, up to that time, being from the 9th April up to the 17th. . . . . Mountains which in ordinary years had their snow-line, at the end of April, at an elevation of 12,000 feet above sea-level, were mantled with snow down to 9,000 feet, and valleys which, at that time of year, used to be clear of all snow up to 10,000 feet, were literally choked with snow down to 6,000 feet; this accumulation in the valleys being caused by snowdrifts and avalanches from the mountain sides."

Messrs. Hill and Archibald's Law.—Meantime evidence of a very important character bearing on the subject in hand, was brought to light from a different quarter. Shortly before the above report was completed and sent to press, Mr. Douglas Archibald* and Mr. J. A. Hill,† in discussing the rainfall of certain stations in Northern India in connexion with the periodical sun-spot variation, a question attracting much interest at the time, had independently arrived at a conclusion which, although reached from a very different starting point and empirical in form, obviously tended to confirm the view above set forth, and indeed, seemed to find its rational explanation in that view. This was that "the winter rains of Northern India are generally heavier when the total fall of the year is below the

* "Nature," vol. xvi, p. 339.

† Report to the North-West Provinces Government.

mean than when the summer rains are excessive." In an elaborate paper subsequently drawn up by Mr. Hill and published in 1879, in vol. i, Part 3, of the "Indian Meteorological Memoirs," he has rediscussed the subject, at considerable length, and has put forward his matured views based on much more extensive evidence. Instead, therefore, of quoting further from his or Mr. Archibald's original writings, I will take the statement of his conclusions as set forth in the postscript to his later paper. This is as follows:—*These results strongly support my original conclusion, that the winter rains are heaviest when the summer rains are defective, and *vice versâ*. They also support the idea put forward by Mr. Blanford, in his report for 1876, that an unusual amount of precipitation over Northern India and the Himalaya, in winter and spring, may, by modifying the normal distribution of pressure, cause the rainfall of the succeeding summer to be defective in the Gangetic Valley, though of course they throw no light on the question of the influence of the Himalayan snowfall on the meteorology of the Bombay Presidency.

"Thus the following twelve years had the winter rainfall excessive, and that of the summer defective, viz., 1845, 1850, 1851, 1852, 1853, 1859, 1865, 1866, 1868, 1869, 1877, and 1878; and the thirteen years 1846, 1847, 1854, 1856, 1858, 1861, 1862, 1863, 1867, 1871, 1873, 1874, and 1875, had dry winters followed by wet summers. Against these twenty-five instances in favour of the rule, we have only three years, 1855, 1870, and 1872 with the rainfall excessive at both seasons, and six years, 1848, 1849, 1857, 1860, 1864, and 1876, with a deficiency both in summer and winter."

The Contrast of the Mountain Winter Rainfall and Summer Rainfall on Plains.—Mr. Hill's conclusion, as above stated, is based on a comparison of the winter rainfall (November to April) of the North-Western Provinces, with that of the summer (May to October) in the same area, and the statistics of the former term of comparison, as well as those of the latter, are furnished therefore chiefly by stations on the plains of Northern India. In general, no doubt, precipitation on the mountain zone varies more or less *pari passu* with that on the plains. But still, this is only true as a rough generalisation. The terms involved in the hypothesis now under discussion are strictly and rigorously, on the one hand, the winter and spring snowfall on the hills, and on the other, the summer monsoon rainfall on the plains. Moreover, Mr. Hill has included in the summer rainfall that of the month of May. As regards the plains this is perhaps admissible, but in the North-Western Provinces the May rainfall is not, strictly speaking, that of the summer monsoon rains. It is either due to diurnal storms, or to what is known as the *chhota barsát*, or little rains, a burst of rainy weather, which frequently precedes the

* "Indian Met. Mem.," vol. i, p. 209.

regular rains, and is followed by a fortnight or more of dry hot weather. On the hills, as occurred in the present year (1883), it frequently takes the form of snow on the higher ranges, and when very copious is rather prejudicial to the monsoon rains.

In the following table, therefore, I have taken as representing the winter and spring rainfall, the total from January to May inclusive at twelve stations, either on the Himalaya or situated immediately at its foot. The table gives the annual precipitation since 1864 (as far as it is on record) in the form of percentages of the local averages for the same season, and the last line but one is the means of the figures in each column. The last line shows the subsequent summer rainfall of the North-Western Provinces, also in the form of a percentage of the general average of that season; the figures up to 1878 being taken from Mr. Hill's memoir.

As might have been anticipated from the stricter character of the test now applied, the results shown in the above table are even more favourable to the hypothesis than those educed by Mr. Hill. Of the eighteen years enumerated in the table, fourteen give confirmatory and only four adverse evidence. And of these four years, one (1876) is that the conditions of which originally suggested the hypothesis as already recounted above, and another (1880) apparently the most discrepant of all, I shall presently refer to, as affording in the very anomalies two of the most striking illustrative instances of its validity. The remaining two exceptional years, 1866 and 1872, I have no means of satisfactorily investigating.

The year of the heaviest winter and spring rains, shown in the table (1877), is that already referred to as one of an almost unprecedentedly heavy snowfall in the Himalaya, and it is also that of the smallest summer rainfall of the eighteen years. The two years which come next in order of winter raininess, viz., 1878 and 1865, had comparatively but a small deficiency of the summer rainfall. But it is certain that the Himalayan snowfall in the early months of 1878 was far from being comparable with that of the previous winter. Nevertheless, the rains were much retarded, so much so, as to give rise to serious anxiety. In the North-Western Provinces there was scarcely any heavy rain before the 6th or 7th of July, and after a few day's fall, a break set in which lasted up to the end of the month.*

The experience of this year, and also that of 1876, and, as will presently be shown, that of 1880, proves then, that the copiousness of the winter and spring rainfall on the outer Himalaya is by no means an exact criterion of that of the snowfall on the higher ranges; and it is only appealed to in evidence, in the absence of any regular reports on the snows, with which we are more immediately concerned. But apart from this consideration, I may here observe, that there was

* See Report on the Meteorology of 1878, pp. 135–137.

Annual Rainfall of the North-Western Himalaya from January to May as percentages of the local averages of that season, compared with the summer monsoon rainfall of the same years in the North-Western Provinces and Oudh; also as percentages of the season's average.

	1864.	1865.	1866.	1867.	1868.	1869.	1870.	1871.	1872.	1873.	1874.	1875.	1876.	1877.	1878.	1879.	1880.	1881.
Peshawar	67	144	106	81	102	76	39	103	123	105	102	82	105	194	182	53	15	119
Abbottabad	150	136	88	119	148	123	47	94	159	66	87	86	81	165	157	44	39	127
Murree	..	..	..	..	..	..	54	96	138	72	92	86	99	182	143	35	34	171
Rawalpindi	130	97	96	94	82	168	17	61	137	74	107	57	140	182	190	31	43	92
Dharmsala	108	184	67	90	151	106	53	89	74	81	107	75	86	178	149	66	79	101
Simla	207	161	56	75	114	68	82	79	105	72	91	96	89	153	150	48	101	123
Chakrata	..	..	..	..	..	101	86	51	65	64	58	94	106	200	183	68	125	100
Dehra	100	259	109	94	164	124	108	67	157	56	133	151	49	177	185	59	114	86
Ranikhet	..	..	..	..	..	..	..	..	..	..	..	..	94	87	247	60	84	130
Naini Tal	80	158	95	77	146	111	71	102	105	82	53	105	40	180	113	64	84	92
Almora	64	156	100	86	131	122	88	116	122	80	96	112	100	147	185	67	85	122
Paori	85	161	83	67	146	92	81	110	135	78	67	73	58	204	129	54	89	85
Means January to May	110	162	89	87	131	101	66	88	120	75	90	99	84	171	168	54	74	112
Mean monsoon rainfall in the N.W. Provinces.	69	95	92	135	66	97	121	131	111	101	131	14	93	45	90	134	69	98

another condition which influenced, in a very important degree, the meteorology of the years 1876 to 1878, which may indeed have originated in conditions more or less similar to the above, but was of far wider incidence, and must have been determined by circumstances operating far beyond the limits of our area. This condition was that excessive atmospheric pressure which, from August, 1876, to August, 1878, prevailed continuously over India, and which I have noticed at length in the Report on the Meteorology of India in 1878.* In India this was at its maximum from July to September, 1877, and its disappearance in Northern India in August, 1878, about coincided with the abundant influx of the monsoon rains of that year. In Southern India it had disappeared earlier, viz., in May. During the northern summers of 1876 and 1877 it was even more excessive in extra-tropical Asia on the one hand, and in Australia (in the southern winter) on the other, than in India. It was probably the presence of this condition in the monsoons of 1876 and 1877 and its absence in the latter part of that of 1878 which in part, at least, determined the differences in the rainfall of these years. Still, this is no argument against the local influence of the Himalayan snows, for which there is ample independent evidence. It only shows that the latter were not the sole cause operating in those years.

To return to the consideration of the facts shown by the table. The two years with the smallest winter rainfall, viz., 1879 and 1870,† when it amounted respectively to only 54 and 66 per cent. of the general winter average, were years of very plentiful summer rains, that of 1879 being one of the highest on record, and one-third in excess of the average, while that of 1870 was as much as 21 per cent. in excess. In three other years, in which the excess of the summer rains amounted to as much or nearly as much as in 1879, the winter rainfall on the North-Western Himalaya had been from 10 to 13 per cent. below the general average.

Evidence of the Exceptional Year, 1880.—The facts of the deficient rainfall of 1880, both in the winter and summer seasons, are of especial interest in relation to the present discussion. In this year the winter rainfall was on the whole considerably below the average, but in February there was a heavy fall of snow on the North-Western Himalaya, when according to a report received from the Commissioner of Kumaon, General Sir H. Ramsay, " the fall of snow in Almora came down to a lower altitude than it had been known to do for many years."‡ Further, with respect to the mountains around

* *Op. cit.*, pp. 53–58.

† It is noteworthy that Mr. Hill adduces 1870 as a year of winter rainfall above the average, and therefore as affording adverse evidence. This is, of course, due to the different method in which he has treated the data.

‡ Report on the Meteorology of India in 1880, p. 166.

the sources of the Ganges, the report states "The winter has been an exceedingly severe one; snow fell at Srinagar (in the Ganges valley), Ramri, Palinda (a few miles north of Katawara), places where it was seldom seen." The months of March and April which followed were of excessive dryness. Scarcely a drop of rain fell in these two months on the plains of the North-Western Provinces, the Punjab, Rajputana, or Central India, and in May, June, and July, except in the Punjab and the most northerly of the adjacent districts of the North-Western Provinces, the fall was considerably below the average. But in June and July there was heavy rain in the Eastern Punjab and on the North-Western Himalaya. In July, "on the hills the rain was almost continuous; at Dharmsala, there was but one day in the month on which no rain was registered, and at Simla but seven days."* The rain was equally heavy in the hills of Kumaon and Garhwal, and as appeared from information subsequently received the precipitation extended far into the heart of the mountain zone. Major Biddulph, then stationed at Gilgit, in the north-west of Cashmere, wrote to me to the effect that the weather was unusually stormy in Northern Cashmere throughout July, and that "the rain and snowfall on the mountains have been such as are unprecedented at this season." Also a letter from the Reverend Mr. Heyde, stationed at Kailong in Lahoul, reported "such weather as Major Biddulph experienced in July of this year . . . . we certainly had also in Lahoul, about the same time. It commenced on the 1st July with heavy rain, which, after a few hours, changed to snow, and lasted till the 6th. The snow fell heavily for three successive days, doing much harm. Ponies, cattle, sheep, and goats grazing on the higher hills perished in large numbers in the snow, &c." This unseasonable snowfall was followed, in August, by an almost entire suspension of the rains on the plains of North-Western India. As described in the official report on the meteorology of the year, "August was conspicuously a dry month in India, and to some extent in Burmah. Indeed the last half of the month was almost rainless in Northern India; and dry westerly winds set in, which recalled, for the time, the disastrous seasons of 1876 and 1877, and seemed to justify the most gloomy forebodings." In the Western Punjab there was no rain, and in the Eastern Punjab and the North-Western Provinces, except in the eastern districts and in Kumaon, it was practically rainless after the 12th or 14th. In Rajputana and the States of Central India, although the rainfall of the month was very deficient, the deficiency, except in the eastern States, was much less than on the Gangetic plain and in the Punjab. Even in the Central Provinces, with one or two exceptions, the fall was very deficient.

It is not then too much to say that this very exceptional year (as

* Report on the Meteorology of India in 1880, p. 149.

tried by the rough test of contrasting the winter rainfall on the hills with the summer rainfall on the plains) affords some of the most striking evidence in support of the hypothesis that the snows favour the production of dry land winds on the plains, and is eminently one of those exceptions that tend to prove the rule.

Negative Evidence of the Years 1881 *and* 1882.—I may pass rapidly over the years 1881 and 1882, which presented no feature of striking importance. The winter of 1880–81 was one of light snowfall. There was a moderate fall in March, 1881, and the succeeding April and May were dry and seasonable, characterised chiefly in Rajputana and Central India by hot land winds of somewhat more than usual steadiness.* The rains set in and came to an end somewhat earlier than usual, but the rainfall, with merely local exceptions, was well up to the average, and in some parts of Northern India somewhat above it.

The winter of 1881–82 also was on the whole dry and mild. In Kumaon, Sir H. Ramsay reported it as "unusually mild;" and Mr. Ney Elias, writing from Leh, on the 5th April, stated that, "the snow has been extraordinarily light this year in Ladak, and the winter altogether unusually mild . . . . Even high altitudes are much less covered than usual." But he adds, "I hear from Dias that the snow on the outer range (at and near the Yogi pass) is as heavy as ever. In fact, the snowfall is very partial and irregular in these mountains." The rains set in early and copiously on the Bombay side; and up to the end of July, in Western and North-Western India, were even excessive; but a change took place in August, and though there was no period of actual drought, the fall during a part of August was light throughout the north-west of India, and notwithstanding the recurrence of heavy rain in the latter part of the month, the total fall of the season was somewhat below the average in the North-Western Provinces, and more so in the eastern than in the western districts.

The Year 1883. *Forecast of Drought and the Result.*—In previous years, the available information respecting the snowfall had been meagre, and had had reference to only one or two districts in the Himalaya. But having now been convinced of the high importance of the subject, I took steps, by private correspondence, and officially with the aid of the local governments, to obtain more numerous and detailed reports. In December, 1882, and the early part of January, 1883, the season was fine and warm; but on the 10th the ordinary cold weather rains set in in the upper provinces, and snow fell at Taini-Tel and some other stations. During the remainder of the month the falls were frequently repeated on all parts of the North-Western Himalaya, and from the night of the 24th to that of the

* Report on the Meteorology of India in 1881, pp. 85, 86, 111, 171, 175, &c.

27th snow fell almost without intermission, and down to levels at which it has rarely been known. At Simla it accumulated to a thickness of 5 feet, on surfaces free from drifts, and even at Rawalpindi, at a distance from the mountains, on the plain of the Patwar, and only 1700 feet above the sea, it lay to a depth of 4 inches on the night of the 27th, an occurrence stated to be without precedent in the memory of the inhabitants. At Murree, the Deputy Commissioner reports the total aggregate fall of January to have been 26 feet, but this seems to require confirmation, if by this statement it is implied that the measurement was made in undrifted snow. At Chamba it was stated to be 9 or 10 feet.

The falls were repeated frequently in February and March, though less heavily, and these additions to the unmelted residue of the previous accumulation caused the mountains to retain their snow mantle down to low levels at a time when, in years of less abundant precipitation, it is restricted to the greater altitudes and occasional hollows at 12,000 or 13,000 feet. The area of the unusual fall was very extensive. It included Kumaon and Garhwal, Sirmoor, and a portion of Bissahir, Kangra, and Hazara. But in the valleys and passes to the north-east and south-east of Wangtu (in the Sutlej valley) the fall was small as compared with former years.* So also at Sultanpur and Plach in the Kulu valley, the fall was considered to be below the average, and in Lahoul and Spiti it was unusually light. In Chamba the fall was abnormally heavy on the first snowy range between the Ravi and Chenab, but on the north-east of the range it was light, and "the Pangi men complained that they had not had enough snow."† In Hazara, the Deputy Commissioner reported that "the snowfall is unprecedented since 1877 (the winter previous to the scarcity of 1877–78);" and Colonel Sir Oliver St. John, writing from Jamu on the 28th February, informed me "there was a heavy fall of snow in the Kashmir valley, and over the Pir Punjal range at the end of last month, but I do not learn that it was anything out of the common." Also Mr. Ney Elias, writing from Leh, states that "last winter was a very open one in Ladak, and both ranges and valleys were unusually lightly covered till the beginning of May."

These reports show that the abnormal snowfall was restricted to the outer Himalaya and the first snowy range; but over this region it was very thick and lasting. During the latter part of January and in February and March, the weather over a great part of Northern India was exceptionally cold. But the temperature rose in April, and May was a hot and remarkably dry month up to the 21st. The dryness was greatest on the North-Western Himalaya; the relative

* As reported by Mr. G. G. Minniken, Assistant Superintendent of the Hill States.

† In a letter from Major C. H. Marshall, Superintendent of the Chamba State.

humidity of the air at Chakatra on the 19th and 20th, at 10 A.M., being only 8 per cent. of saturation (according to the telegraphic report), or 45 per cent. below the general average of the month of May. Great heat and dryness prevailed also with the westerly winds over the whole of extra-tropical India, with the exception of Bengal and Assam.

In Ladak, however, very bad weather set in in the beginning of May, and lasted nearly a month. This thickened the snows, and for a time brought the snow-line down to about the ordinary March level. In the last week in May the weather again became clouded over Northern India and the outer Himalaya; the temperature fell, and on the 28th and 29th another heavy fall of snow took place on the mountains, whitening their sides down to 10,000 or 11,000 feet.

In was under these circumstances that the following notes were published in the "Gazette of India" (2nd June). The first was written on the 18th May, when the dryness of the air was about at its maximum, and a day or two before the change set in that culminated in the snowfall of the 28th and 29th May. The second was written on the 31st May, immediately after the fall.

I. "That the unusually dry weather now prevailing over the North-Western Himalaya, and that which, though less abnormal, characterises the whole of North-Western India at the present time, is an effect of the unusual accumulation of snow, is a conclusion justified by the experience of the last few years; and were it not that the snow is rapidly decreasing under the unobstructed radiation of the sun, there might be some reason, judging from the present limited experience, to anticipate some retardation of the rains in the upper provinces, and possibly even in Western India generally. But, on the other hand, the fact that during the months of April and May the atmospheric pressure over the greater part of the country has been below the normal average of the season, is one which . . . . portends favourably for the timely influx of the monsoon. In Bengal it may be said the prospects are wholly favourable."

II. "Since the above was written there has been heavy rain for many days on the outer hills, and more or less on the plains of the Punjab, and apparently a very heavy fall of snow on the higher ranges . . . . If, therefore, the mountains of Lahoul, Spiti, and other more distant ranges have shared this fall, if it is as extensive as it is apparently heavy on the visible ranges, and if the views which the experience of recent years seem to justify, viz., that an unusual extent and thickness of snow on the Himalaya is productive of dry north-west and west winds in North-Western India are valid, we must be prepared for a long spell of dry weather and a retarded rainfall in the upper provinces."

June was a very dry month in the upper provinces and Rajpu-

tana. In Bengal the rains set on in the 13th, which is a day or two in advance of the average date, but they extended inland only as far as Behar, with a moderate fall up to Allahabad. Further westward there were occasional thunderstorms, but no heavy rain fell before the 26th, nor in the Punjab before the 29th, and even then it was not continuous. On the 2nd or 3rd July, in writing a summary of the weather of the previous month, Mr. Dallas remarks:—"In parts of the North-Western Provinces and the Punjab continuous rain has hardly yet set in."

In Rajputana there was scarcely any rain in June, but the month was decidedly cool; and in Bombay and Gujarat also the temperature was below the average. In Bombay the monsoon did not set in steadily before the 24th, and the rainfall was light throughout the month. This coincidence of deficient rainfall, and a temperature below the average, is unusual.

During the first half of July rain was frequent and heavy on the North-Western Himalaya; but, according to my own observations at Simla, this rainy period presented one or two noteworthy features. In the first place, thunderstorms were repeated day after day; and, secondly, the rain was frequently accompanied with hail. These accompaniments are characteristic of the storms that precede the rains, and are exceptional in a rainy season of the normal character. As far as my experience goes, they indicate an unsteady monsoon or its approaching termination, and the existence of a dry current at no great elevation above the rain clouds. Certain it is, that at frequent intervals during this rainy period the existence of such a current was rendered evident by the drift from the north-west of small broken cloud tufts, or by the inclination and movement of the tops of the rain clouds, when the lower current was from some southerly direction, generally up-valley.

About the 19th July the rains ceased, except for occasional thunderstorms, and from that time to the end of August this north-west current held full sway, frequently down to the surface of the hills (7,000 feet), but more generally perhaps 2,000 or 3,000 feet higher. This same current prevailed also throughout the monsoon season of 1877, and during the drought of August, 1880; and it is also characteristic of the spring months. It appears to me to be the feeder of the westerly and north-westerly winds (the land winds of North-Western and Western India, as I shall presently explain more at length).

On the plains of the Punjab (except in the eastern districts, between the 6th and 18th July) there was no continuous rain, but heavy falls, for the most part very local, occurred at intervals up to the 19th. After this date, up to the end of August, save on two days, there was no fall amounting to 1 inch in any part of the province; and the districts east and south of Ludhiana, and all to the west of

Lahore and south of the salt range, were absolutely rainless. In Rajputana there was no rain of any importance from the 18th July to the 29th August, and the absolutely rainless interval was from the 6th to the 17th August. In the North-Western Provinces all the western districts were rainless from the 26th July to the 20th August; and in Oudh and the eastern districts, except in Mirzapur, there were only rare and insignificant showers. Still further east, in Behar, showers were somewhat more frequent, but still only occasional, and for the most part light. In Gujarat, Khandish, and Berar, the drought was equally great and prolonged; lasting from the 18th July to the 26th August, and at some stations up to the 31st. And even in Hyderabad and the Deccan there was no general rain, and but few heavy showers locally, for the space of an entire month, from the 20th July. In the Konkan light showers fell daily up to the 7th August; but even here and on the Ghats the rainfall was very light, and the interval from the 8th to the 16th August was almost rainless.

The extent of the drought was therefore very great, including the whole of Western and North-Western and the greater part of Central India. It began earlier and ended later in the Punjab and Northern Rajputana than further to the south and east, and was most severe during eight or ten days in the middle of August. It affected nearly the whole of the country in which the land winds prevail most regularly and persistently during the spring months; and the winds during the drought were identical in character and direction with those which are normal in the spring season.

During the period here dealt with, the barometric features, though subject to considerable variation, were less anomalous than might perhaps have been anticipated. In April there had been, on the whole, a slight excess of pressure above the average on the Punjab Himalaya, about an average pressure on the plains of the greater part of the province, and a slight deficiency on the Gangetic plains and the adjacent hills, from the Jamna eastwards, greater on the plains than on the hills. In May, save in the north of the Bombay Presidency during the first week, and generally on the last three or four days, the pressure was below the average throughout Northern India, the deficiency averaging, on the mean of the whole month, $-·038''$ on the North-Western Himalaya, $-·041''$ on the Punjab plains, and $-·056''$ on the Gangetic plain down to Benares. To the south of the Ganges, in Central India, Rajputana, and the Central Provinces, it was less, averaging from $-·020''$ to $-·030''$. With the snowfall at the end of May the pressure rose greatly above the average both on the hills and plains, and so remained nearly a week. About the 4th June it fell again, and remained unduly low over the whole of Northern India till after the middle of the month. Between the 18th and 20th, that is, a few days after the rains had set in

in Bengal, it again rose above the average, and except for another fall on the 27th and 28th, just before the rain, it remained excessive during the latter part of the month (save, indeed, in the Western Punjab), the excess culminating on the 21st and 25th. On the plains of the Western Punjab the temperature was very high; at Rawalpindi, on some days, as much as 10° or 12° above the average of the month; which fact probably explains the local anomaly of the average pressure in that region.

The high pressure thus established in the latter part of June lasted in the Eastern Punjab and the North-Western Provinces, with but slight intermission, throughout the first half of July, being very excessive at the beginning and end of that period. To the eastward in Bengal, and to the south-east and south in the Central Provinces and Bombay, as well as in the Western Punjab, the oscillations were similar in phase, but the average excess was either less pronounced or altogether nullified and reversed, the pressure being above the average only for a few days at the beginning and after the 15th of the month. An abnormal excess of pressure relatively to the remainder of Northern India was therefore existent in the Eastern Punjab and the North-Western Provinces.

In the latter part of the month, after the cessation of the rain, the pressure fell generally below the average in Northern India, and especially in the Punjab and North-Western Provinces, but the northern part of Bombay formed an exception to the rule. A great deficiency of pressure both on the hills* and plains characterized also the first days of August, and on the plains and at the westernmost hill station, Murree, this was associated with an excessive temperature; but at Simla, Chakrata, and Ranikhet, the latter was more or less considerably below the average. After the 6th the pressure rose again, and smaller oscillations, lasting five or six days, followed during the remainder of the month. In the Punjab, the North-Western Provinces, and Bengal, the mean pressure of the month was slightly below the average of many years. In Rajputana, Gujarat, Sind, and Western and Central India generally, such was not the case, since in this region an excessive pressure prevailed throughout the middle of the month for seventeen or eighteen days after the 6th. This coincided with the period of the greatest and most extensive drought.

At the hill stations the oscillations of pressure were similar to those on the plains, though less in amount. At Simla and Murree the mean pressure of the two months, July and August, scarcely deviated from the normal average. But at Leh in both months there was a decided deficiency, and it was but slightly less at Chakrata and Ranikhet.

* That is at levels of about 7,000 feet.

The barometric features of the season may then be summed up as follows:—With local and more or less temporary exceptions the pressure of the atmosphere over India was below the average throughout the first eight months of the year, the greatest depression being in May, when it was universal in India. But this was only the general average of a series of oscillations of pressure (somewhat greater than usual at most stations in May, June, and July), which succeeded each other throughout the period. These oscillations were closely connected with great variations of temperature, and were to some extent an effect of the latter, each fall of rain (or snow on the hills) being accompanied with a rise of pressure and a temporary excess which speedily diminished, and was followed by a fall below the average, with the return of dry, fine weather.

Meteorology of the Land Winds.

The Westerly Winds of the Winter and Spring.—The land winds from west and north-west are the characteristic winds of the winter, spring, and early summer throughout the greater portion of Northern India, and of Western India from February to May. In the Punjab indeed, the atmosphere is most frequently calm, or agitated only by light movements of very variable direction,* and especially so in the winter months. But down the Gangetic plain (save in the neighbourhood of the hills), and on the plateau of Rajputana and Central India, light and steady winds from between north-west and west are those most characteristic of the whole of the dry season. During the winter months these winds are cooler, from March onwards warmer, than those from the opposite quarters; and in April and May they constitute the well-known hot winds of Northern India. At both seasons they are dry winds, and the striking change in their temperature is due, as was long ago pointed out by Sir Joseph Hooker, to that of the land surface swept by them. *Pari passu* with the rise of temperature they undergo certain partial changes of direction, and also in the region of their prevalence. From November to February, when, as a rule, an axis of maximum pressure runs from the Punjab and Sind across Rajputana, Central India, and the Central Provinces towards Orissa, with lower pressures over the Gangetic valley and Bengal on the one hand, and over the peninsula, and especially on its south-west coast, on the other, the current of which they form part tends to circulate anticyclonically around the ridge of high pressure above defined, and thus the directions are northerly on the eastern portion of the Central India plateau and easterly to the south of the Satpura chain in Nagpur, the Deccan and Hyderabad. But when in March a barometric minimum is established over the

* See "Winds of Northern India," "Phil. Trans." for 1874, vol. 164, p. 563.

Bellary and Hyderabad plateau, which speedily extends to the eastern half of the Central Provinces, Chutia, Nagpur, and the Gangetic plain, the winds, in accordance with the law of cyclonic circulation, become northerly or north-westerly in the Deccan, and the western half of the Central Provinces and Central India, and almost due west on the plateau south of the Ganges and Jamna, and on the plain along the course of those rivers. On the other hand, in accordance with the same cyclonic law, the winds of the east coast and maritime plains of the peninsula and Bengal are chiefly from south; and as the season advances, the current from the sea creeps up the northern margin of the Gangetic plain as a south-east wind under the lee of the hills and intermittently sweeping the crests of the outer Himalaya.

This distribution of the winds explains that of the spring rains, which fall, chiefly in thunderstorms, on the southern and eastern districts of the peninsula and in Bengal; whereas, westward from Nagpur, in the Deccan, Berar, Khandesh, Gujarat, and even the Konkan, Kátywár, and Cutch, indeed the whole of Western India, southwards to Belgaum and northwards to the confines of the Punjab, the season from November to May is practically rainless. This latter is the realm of the dry land winds, or, as regards the coast, northerly long-shore winds; this being the local phase of the general current, and equally unfavourable to precipitation.

To one portion of this region, viz., Sind, Cutch, Kátywár, and the adjoining portion of Western Rajputana, the above description of the winds is applicable only with some modification. In this part of Western India, north-east is the prevailing direction from November to February, and in the subsequent months south-west winds are frequent even if they do not preponderate. But these latter bring no rain. They appear to be an indraught from the coast, and as they penetrate the country they coalesce with the general stream and contribute their quota to the dry winds of Eastern Rajputana and Gujarat. They become merged and lost in the prevailing current.

The question then presents itself, "What is the origin of the dry westerly current? The supposition that the indraught from the south-west furnishes more than a small portion of the stream is at once negatived by the fact that, even at Kurrachee and Bhuj, southerly winds do not preponderate over northerly until May, and even then almost inappreciably; at Rajkot, not before June; and the very fact of the great dryness of the west and north-west winds militates against the idea that any considerable portion of their air mass can be drawn from the sea. Neither is it derived to any considerable extent from the valleys and lower slopes of the surrounding hills. There is no permanent drainage of air from these hill slopes, and strong winds blowing outwards from the longer valleys, like the

dàdu of Hurdwar, are local and exceptional phenomena, restricted to certain hours of the day. At all the hill stations of the outer North-West Himalaya, as far as the existing registers show, southerly winds preponderate over northerly, all through the year; and although this is probably due in some measure to the fact that the night winds have not hitherto been registered, it suffices to show that up to a level of 7,000 feet there is no steady outflow of air from the hills to the plains.

There remains then only the supposition that these winds are fed by the descent of air from an upper stratum, viz., from a current moving at a considerable elevation from west to east. And that this is their true explanation several facts seem to testify. In the first place, they are characteristically winds of the day time, their movement being at a minimum (almost or quite a calm) in the morning hours, and indeed up to 9 or 10 o'clock in the forenoon; then increasing with the temperature and falling again towards evening; and, secondly, such observations as have been made on the decrease of temperature with elevation, show that, in the dry weather, the vertical decrement is such as is incompatible with the vertical equilibrium of an air column, being considerably more than 1° in 183 feet. The diurnal variation of the movement is then probably to be accounted for on Köppen's hypothesis, viz., the interchange of the higher and lower air strata by convective movements, which do not affect the existing horizontal movement of the higher atmosphere, so that the air of the latter, after its descent, preserves for a time its original eastward motion. The hypothesis of convective interchange receives further support from the character of the diurnal variation curve of vapour tension in a dry atmosphere near the earth's surface, which is the same in all parts of India. This shows a rapid fall of the absolute humidity of the air after 8 or 9 o'clock in the forenoon, reaching its minimum about the time of greatest heat, and a more or less sudden rise before sunset which it is difficult to account for on any other supposition than that it coincides with the cessation of the convective movement.*

Systematic observations on the movement of the clouds, and more especially the higher clouds in the upper provinces during the dry season, are unfortunately as yet wanting, but according to such casual observations as I have myself made when on inspection tours in the cold season and during the spring months or hot season at Simla, go to show that the movement is generally, if not always, from some westerly quarter, and most frequently from the north-west.

* See, for instance, Plates II, VIII, and XXVI of vol. i of these memoirs. The curves of Yarkhand and Allahabad show that this type of variation is characteristic of the months in which the range of temperature is greatest.

The North-West Winds after Winter Rainfall.—A phenomenon which almost invariably follows a fall of rain and snow on the North-Western Himalaya in the winter and early spring months, and which has been repeatedly described in the Annual Reports on the Meteorology of India,* is a wave of high pressure advancing eastward from the valley of the Indus, accompanied with steady cool north-west winds on the plains. Charts illustrating this phenomenon have been given in several of the Annual Reports on the Meteorology of India, from which I select that of February 26th, 1881, as a very characteristic example (fig. 1). At this season, the snow falls at comparatively low levels (occasionally, though rarely, as in the present year, to below 2,000 feet), and below the snow limit the hill slopes and valleys are cooled by the rainfall.

In these cases it can hardly be questioned that the north-west wind is simply an outflow of cold air from the hills, the high density of which is the chief cause of the rise of the barometer. On the plains, in the neighbourhood of the hills, it rarely lasts more than a few days; not longer indeed than it requires to melt the low lying snow and to evaporate the fallen rain, but the phenomenon is of great interest in the bearing that it has on the main topic of this paper, affording an illustration at a low level of that which I conceive to operate at a high level on a more lasting and extended scale, later on in the season.

Summary and Conclusion.

The principal facts and conclusions set forth in this paper are as follows:—

1st. The experience of recent years affords many instances of an unusually heavy and especially a late fall of snow on the North-Western Himalaya being followed by a prolonged period of drought on the plains of North-Western and Western India.

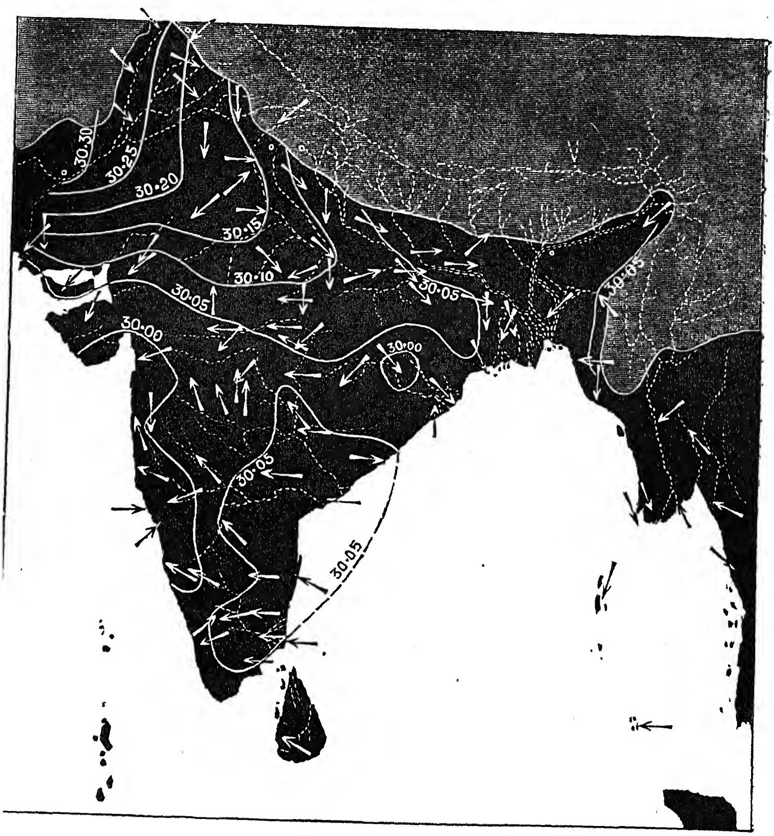
2nd. On tabulating the average rainfall of the winter and spring months at the stations of the North-Western Himalaya, year by year, for the last eighteen years, and comparing it with the average rainfall of the North-Western Provinces in the ensuing summer monsoon, it is found that with four exceptions an excessive winter precipitation on the hills is followed by a deficient summer rainfall on the plains, and *vice versâ*. Of the four apparent exceptions, two are found to afford a striking support to the first proposition.

3rd. The west winds which, in Western and Northern India, are characteristic of seasons of drought as abnormal winds, are identical in character with the normal winds of the dry season, and appear to

* See, *e.g.*, Reports for 1878, pp. 129, 130; 1879, pp. 136, 154; 1880, pp. 143, 144, 171; 1881, pp. 151, 152.

be fed by descending currents from the North-Western Himalaya and possibly the western mountains generally.

4th. It is a common and well-known phenomenon of the winter months that a fall of rain and snow on the North-Western Himalaya is immediately followed by a wave of high pressure advancing eastwards from the western mountains, accompanied with dry cool northwest winds.



Baric and Wind Chart for 10 A.M. of February 26, 1881.

5th. The conclusion is that an unusual expanse of snow on the North-Western Himalaya, whether due to the unmelted residue of an unusually copious winter snowfall, or to an unusually late fall in the spring months, acts, at high levels, in the summer months, in somewhat the same way as the ordinary falls of snow and rain on the Lower Himalaya do at low levels in the winter season, and favours

the production of dry north-west winds on the plains of Western India.

6th. That this dependence of dry winds on the Himalayan snow-fall affords a criterion for forecasting the probabilities of drought in North-Western and Western India.

In setting forth the above conclusions, it is, however, necessary not to ignore the fact that there other conditions besides those here considered which exercise a very great influence on the prevalence of dry winds and drought. During the last famine period in India (the years 1876 and 1877; in the former year in Southern India, in the latter in the North-Western Provinces and Rajputana), the pressure of the atmosphere was persistently and abnormally high, and this was due, as I showed in the reports on the meteorology of those years, to the condition, probably the high density, of the higher atmospheric strata. Moreover this excessive pressure was shown to affect so extensive a region, that it would be unreasonable to attribute it to the condition of any tract so limited as a portion of the Himalayan chain; and if dependent on the thermal conditions of the surface, which may indeed have been the case, this land must rather have been the major portion of the Asiatic continent than merely a relatively small portion of its mountain axis. This question must remain for future inquiry. It is referred to here to guard against too wide an application being assigned to the action of the Himalayan snows.

III. "Report to the Solar Physics Committee on a Comparison between apparent Inequalities of Short Period in Sun-Spot Areas and in Diurnal Temperature-Ranges at Toronto and at Kew." By Balfour Stewart, M.A., LL.D., F.R.S., and William Lant Carpenter, B.A., B.Sc. Communicated to the Royal Society at the request of the Solar Physics Committee. Received April 21, 1884.

(Abstract.)

It has been known for some time that there is a close connexion between the inequalities in the state of the sun's surface as denoted by sun-spot areas and those in terrestrial magnetism as denoted by the diurnal ranges of oscillation of the declination magnet; and moreover the observations of various meteorologists have induced us to suspect that there may likewise be a connexion between solar Inequalities and those in terrestrial meteorology.

This latter connexion, however (assuming it to exist), is not so well

established as the former, at least if we compare together Inequalities of long period. It has been attempted to explain this by imagining that for long periods the state of the atmosphere as regards absorption may change in such a manner as to cloak or diminish the effects of solar variation by increasing absorption when the sun is strongest and diminishing absorption when the sun is weakest.

On this account it seemed desirable to the authors to make a comparison of this kind between short-period Inequalities, since for these the length of period could not so easily be deemed sufficient to produce a great alteration of the above nature in the state of the atmosphere.

The meteorological element selected for comparison with sun-spots was the diurnal range of atmospheric temperature, an element which presents in its variations a very strong analogy to diurnal declination-ranges.

There are two ways in which a comparison may be made between solar and terrestrial Inequalities. We may take each individual oscillation in sun-spot areas, and find the value of the terrestrial element corresponding in time to the maximum and the minimum of the solar wave. If we were to perform this operation for every individual solar Inequality, and add together the results, we might probably find that the magnetic declination-range was largest when there were most sun-spots. If, however, we were to make a similar comparison between sun-spot daily areas and diurnal temperature-ranges we might not obtain a decisive result. For at certain stations, such as Toronto, it is suspected (the verification or disproval of this suspicion being one of the objects of this paper) that there are two maxima and two minima of temperature-range for one of sun-spots. The effect of this might be that in such a comparison the temperature-range corresponding to a maximum of sun-spots might be equal in value to that corresponding to a minimum, or, in other words, we should get no apparent result, while, however, by some other process proofs of a real connexion might be obtained. But if we can get evidences of apparent periodicity in sun-spot fluctuations when dealt with in a particular manner, we have at once a method which will afford us a definite means of comparison. And here, as Professor Stokes has pointed out, it is not necessary for our present purpose to discuss the question whether these sun-spot Inequalities have a *real* or only an *apparent* periodicity. All that is needful is to treat the terrestrial phenomena in a similar manner, or in a manner as nearly similar as the observations will allow, and then see whether they also exhibit periodicities (apparent or real) having virtually the same times as those of sun-spots, the phases of the two sets of phenomena being likewise allied to one another in a constant manner.

It is such a comparison that the authors have made, their method of analysis being one which enables them to detect the existence of unknown Inequalities having apparent periodicity in a mass of observations. A description of this method has already been published in the "Proceedings of the Royal Society" for May 15th, 1879. The comparison was made by this method between sun-spot observations extending from 1832 to 1867 inclusive, Toronto temperature-range observations extending from 1844 to 1879 inclusive, and Kew temperature-range observations extending from 1856 to 1879 inclusive. The following conclusions were obtained from this comparison.

(1.) Sun-spot Inequalities around 24 and 26 days, whether apparent or real, seem to have periods very nearly the same as those of terrestrial meteorological Inequalities as exhibited by the daily temperature-ranges at Toronto and at Kew.

(2.) While the sun-spots and the Kew temperature-range Inequalities present evidence of a single oscillation, the corresponding Toronto temperature-range Inequalities present evidence of a double oscillation.

(3.) Setting the celestial and terrestrial members of each individual Inequality, so as to start together from the same absolute time, it is found that the solar maximum occurs about 8 or 9 days after one of the Toronto maxima, and the Kew temperature-range maximum about 7 days after the same Toronto maximum.

(4.) The proportional oscillation exhibited by the temperature-range Inequalities is much less than the proportional oscillation exhibited by the corresponding solar Inequalities.

IV. "Some New Phenomena of Electrolysis." By G. Gore, F.R.S., LL.D. Received April 23, 1884.

Whilst making a series of experiments on the "self-deposition of metals," I observed, by trying a number of different metals, that several of them received an electrolytic deposit of cadmium by contact with cadmium in various solutions of that metal much more frequently than others; I therefore made various experiments to determine whether this was due to difference of density of current or to other causes.

By means of these additional trials, I found, on passing an undivided current through a series of portions of the same metallic solution, that cathodes composed of different metals of equal amounts of immersed surface, required currents of different degrees of density to cause deposits of the same metal upon them, and that the differences in some cases were considerable. Another singular cir-

cumstance was also observed, viz., that the cathode which most readily received a deposit was frequently the one composed of the same kind of metal as that which was being deposited. I am now examining these new facts.

May 8, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On a Relation between the Coefficient of the Thomson Effect and certain other Physical Properties of Metals." By SHELFORD BIDWELL, M.A., LL.B. Communicated by Prof. FREDERICK GUTHRIE, F.R.S. Received April 23, 1884.

The magnitude and direction of the Thomson effect depend upon a coefficient which is always the same for the same metal, but varies with different metals. Professor Everett, in his "Units and Physical Constants," p. 151, gives a table based upon Tait's thermoelectric diagram ("Trans. R.S.E.," vol. xxvii, p. 125), in which the thermoelectric values of a number of metals, referred to lead as zero, are given in the form $\alpha+\beta t$, where β is a number which for a given metal is proportional to the tangent of the inclination of the line representing the metal in Tait's diagram, and therefore to the coefficient of the Thomson effect.

Since all the physical properties of a metal are to some extent affected by heat, it seemed probable that a connexion might be found to exist between certain of these properties and the Thomson effect. A short examination showed that, as a rule, the coefficient of the Thomson effect is positive in those metals which have a great specific electrical resistance and specific heat, and negative in those which are distinguished by a great coefficient of expansion. I therefore made several attempts to ascertain whether the Thomson coefficient might not be some definite function of the specific resistance, specific heat, and coefficient of expansion; and though I have not been perfectly successful, it appears from the subjoined table that there is a close relation between them.

I. Metals.	II. Coefficient of Thomson effect.	III. Specific heat.	IV. Specific resistance.	V. S.H. × S.R. × 10^6.	VI. Coefficient of expansion.*	VII. Authority.	VIII. $\left(\frac{\text{Expansion}}{34}\right)^2$	IX. S.H. × S.R. × $10^6 - \left(\frac{\text{Exp.}}{34}\right)^2$	X. Last column divided by 2,400
Ni	5·12	0·109	0·126	13,734	1,279	Fizeau	1,414	12,320	5·13
Fe	4·87	0·113	0·098	11,074	1,156	Borda	1,156	9,918	4·13
Pd	3·59	0·059	0·138	8,142	1,104	Matthiessen	1,056	7,086	2·95
Pt (soft) ..	1·10	0·032	0·092	2,944	857	Borda	635	2,309	0·96
„ (hard) .	0·75								
Mg	0·95	0·247	0·031	7,657	2,694	Fizeau	6,273	1,384	0·58
Pb	0	0·031	0·199	6,169	2,799	Matthiessen	6,773	− 604	−0·25
Al	−0·39	0·214	0·029	6,206	2,220	Calvert	4,264	1,942	0·81
Sn	−0·55	0·056	0·109	6,104	2,840	Muschenbröck	6,972	− 868	−0·36
Cu	−0·95	0·094	0·017	1,598	1,780	Borda	2,735	−1,137	−0·47
Au	−1·02	0·032	0·021	672	1,460	Muschenbröck	1,844	−1,172	−0·49
Ag	−1·50	0·057	0·016	912	1,910	Kupffer	3,158	−2,246	−0·94
Zn.......	−2·40	0·093	0·057	5,301	2,976	Matthiessen	7,656	−2,355	−0·98
Cd	−4·29	0·054	0·068	3,672	3,159	„	8,630	−4,958	−2·07

* The coefficients of expansion per degree C. multiplied by 10^8.

The first column contains the names of all the metals given in Everett's table, with the exception of alloys.

The second column gives the numerical coefficients of t in Everett's table; these numbers are proportional to the coefficients of the Thomson effect.

The specific heats in the third column are Regnault's determinations, as given in Pickering's "Physical Manipulation," vol. ii, p. 287.

The specific resistances in column IV (except that of tin) are Matthiessen's, as given in Jenkin's "Electricity" and Pickering's "Physical Manipulation." The resistance of tin is reduced from E. Becquerel's determination, on the assumption that the specific resistance of silver at 15° C. is 0·016. Matthiessen's is considerably higher, viz., 0·134.

The fifth column contains the products of the third and fourth multiplied by 10^6 to get rid of decimals.

The coefficients of expansion in the sixth column are taken from the "Encyclopædia Britannica," 9th edition, article "Heat," by Sir William Thomson. The authorities are given in the seventh column.*

The eighth column gives the squares of the coefficients of expansion divided by 34^2.†

The numbers in the ninth column are obtained by subtracting those in the eighth from those in the fifth column.

In the last column the numbers in column IX are divided by 2,400.

It will be seen that, with one exception, the order of magnitude of the numbers in column IX (proportional to spec. heat × spec. res. × 10^6 − (Exp. ÷ $34)^2$) is exactly the same as the order of those in column II, which are proportional to the coefficient of the Thomson effect. The rate of decrease in column X is, however, not the same as that in column II, the numbers diminishing too rapidly in the upper half of the column and too slowly in the lower half. Although, therefore, it appears very probable that the direction and magnitude of the

* Where more than one value was given the first was always used except in the cases of silver and zinc. The expansions for silver are—Muschenbröck's, 2120; Kupffer's, 1910; Matthiessen's, 1973; those for zinc are—Calvert's, 2200; Matthiessen's, 2976; Fizeau's, 2918. In both cases there is a fair agreement between the second and third values, while the first differs from them considerably. The second values give silver and zinc the same places in column X as in column II; the first would change their order.

† The divisor was so chosen that, while the ratio of the first number to the last in column IX should be as nearly as possible equal to the ratio of the first number to the last in column II, the number corresponding to lead in column IX should at the same time be as near zero as possible. Both conditions could not be exactly fulfilled at once.

Thomson effect in any given metal are dependent mainly, if not entirely, upon the specific heat, specific resistance, and coefficient of expansion of the metal (or upon changes of these properties with changes of temperature), the Thomson coefficient is not exactly given by the expression spec. heat $\times$ spec. res. $\times 10^6 - (\text{Exp.} \div 34)^2$, even if due allowance is made for the uncertainty of the numbers in columns II, III, IV, and VI, and for the fact that some of them may vary greatly with different specimens of the same metal. But I have not succeeded in finding a better expression.*

II. "Experimental Research on the Electromotive Force from Difference of Potential during Diffusion in Tidal Streams." By THOMAS ANDREWS, F.R.S.E., Assoc. M. Inst. C.E., Wortley Iron Works, near Sheffield. Communicated by Professor STOKES, Sec. R.S. Received April 20, 1884.

An examination of the composition of the waters throughout the length of a tidal stream during diffusion of salt and fresh water consequent on tidal action, reveals a very considerable difference in the proportion of saline constituents between the water at the surface and that at the bottom, during certain times of tide this difference amounting sometimes to near 100 per cent., and it may frequently be either much greater or less according to tidal fluctuations.

This fact constitutes the basis of the investigation which the author undertook, to obtain some approximate quantitative measurement of the resultant electromotive force, &c., arising from such difference of potential. It is known that a current is set up when a bar or plate of the same metal is immersed in two dissimilar solutions in contact, one capable of acting readily upon the metal, the other having little or no action on it, the whole forming a circuit.

The current continues until diffusion renders the composition of the solutions uniform, after which a reverse current may not

* With regard to aluminium, which in column X comes between platinum and magnesium, instead of between lead and tin, as in column II, it is possible that the specific resistance given by Matthiessen as 0·029 is too high. I do not know of any other determination professing to be exact, but it is stated in Wurtz's "Dic. de Chimie," p. 129, upon the authority of Deville, to be one-eighth that of iron, which would make it 0·012. This, however, is undoubtedly too low. If it were 0·019, the place of aluminium in column X would be the same as in column II, and if it were as high as 0·026, its place would be between magnesium and lead. I may mention that some experiments of my own show that the coefficient of the Thomson effect in aluminium comes slightly above that of lead, instead of below it. And in the diagram at p. 178 of Jenkin's "Electricity" the inclination of the aluminium line is also shown as positive.

unfrequently be observed arising from the previous unequal action of the solutions on the metal.

The series of groups of metals employed in this investigation, viz., wrought irons, various steels, and cast metals, &c., were especially selected in order to render the research of more practical value.

The two dissimilar solutions used were sea water (from Filey Bay) and distilled water.

The author devised the arrangement described below for carrying out this research.

The experiments were made on large round bars of each of the following metals, of known chemical composition and specific gravity, every bar was $2\frac{31}{32}$ inches diameter, carefully turned and polished quite bright, the metals being each especially prepared throughout for these observations.

A careful selection was made with reference to the percentage of combined carbon, specimens containing the highest and lowest being taken, in order that extreme results in each case might be arrived at. The descriptive terms "soft" and "hard" have reference solely to percentages of combined carbon.

This large size and round form of bar were employed to ensure in the manipulation of the metals as uniform a molecular structure as practicable.

The steel and iron bars in each case were prepared from the same ingot or bloom and sawn into equal lengths when finished, so that the bars of the same metal (turned and finished) were identically of one composition, &c. The same exact care was exercised in the preparation of the cast metal bars.

The chemical composition and specific gravities of the metals are shown in Table A.

Table A.—Analyses of the Wrought Iron, various Steels, and Cast Metals employed.

Description.	Graphitic carbon.	Combined carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Iron (by difference).	Total.	Specific gravity.
Wrought iron	...	none	0·392	·034	·270	·194	99·110	100·000	7·590
"Soft" Bessemer steel....	...	0·150	0·009	·112	·088	·468	99·173	100·000	7·853
"Soft" Siemens-Martin steel	...	0·230	0·014	·100	·075	·698	98·883	100·000	7·856
"Soft" cast steel..........	...	0·450	0·016	·027	·048	·086	99·373	100·000	7·863
"Hard" Siemens-Martin steel	...	0·460	0·107	·023	·075	·972	98·363	100·000	7·845
"Hard" Bessemer steel..	...	0·480	0·121	·096	·089	·684	98·530	100·000	7·838
"Hard" cast steel.........	0·259	*1·190	0·175	·063	·019	·396	97·898	100·000	7·805
Cast metal, No. 1	2·780	*0·390	2·340	·080	·580	·450	93·370	100·000	7·206
,, No. 2...........	2·620	*0·670	1·940	·090	·950	·520	93·210	100·000	7·134

* Combined carbon in these samples was determined by combustion, and in the other samples by the colour test.

Method of Experimentation.

The experiments were conducted as follows, in precisely the same manner in each case for comparison.

For the purposes of this research the cells were constructed so that the diffusion effects, electromotive force, &c., observed should approximate to those obtaining during a tidal period of six hours.

A strong wooden box was divided into two equal compartments, A and B, the partition containing at the lower end a porous diaphragm of chamois leather to allow of a suitable diffusion between the solutions.

Two bars from the same piece of metal (of precisely the same composition), polished bright, and exactly $2\frac{31}{32}$ inches diameter, were placed in the cells exactly at equal distances apart in each case ($1\frac{9}{32}$ inch), one bar in partition A, and the other bar in partition B, the bars being attached to the galvanometer No. 1. The partitions A and B were then simultaneously carefully filled up to a depth of 12 inches, the one with sea water, the other with distilled water, and careful telescopic readings taken of the time changes in the deflections of the galvanometer No. 1, regularly during the tidal period of six hours.

The difference in level between the solutions in the cells, caused by the greater specific gravity of the sea water, assisted diffusion, thus approximating to the current pressures in rivers exerted by the tidal flow.

To render the application of these experiments as practical as possible, the observations of the electromotive force and time changes of resistance during diffusion, were taken at regular intervals of two and a half minutes (the results being summarised in Table B), over tidal periods of six hours, so as to afford an approximation to the effects produced by alternating diffusion of salt and fresh water during the tidal changes of a tidal river.

The galvanometer No. 1 with its accessories, resistance coils, &c., employed in these experiments was a delicate astatic one (by Messrs. Elliott Bros.), suspended needles, large mirrored dial, and it was also arranged to work as a mirror galvanometer (R. of galvanometer 521 ohms at 20° C.).

It was constructed as to resistance, &c., specially to suit the purpose of this research.

The galvanometer was carefully calibrated throughout, on the spot, at the commencement of the research with a Daniell's cell in circuit, and the constancy of the instrument afterwards frequently verified.

Another astatic galvanometer, No. 2 (suspended needles), of lower resistance was used for taking the time changes in the resistance of the cells, by the first fling method as described further on.

The results of the observations giving the E.M.F., &c., during diffusion are recorded in the summary of results on Table B, the E.M.F. being calculated from the ascertained resistances in circuit, in conjunction with the known calibration of the galvanometer No. 1, which was used for this part of the research.

For ascertaining the comparative behaviour of the various metals employed, the author has given the average, together with the highest E.M.F. noted during the observations in each case.

Determination of Resistance of Cells.

Difficulties were experienced in determining the resistance in the cells which was momentarily such an inconstant quantity, owing to diffusion between the two solutions, and the difficulty was further increased by polarisation when the Daniell's cell was connected.

After conferring with Professor J. V. Jones, B.Sc., B.A., recourse was had in separate experiments to the method of rapidly alternating the direction of the current sent through the cells (it being first sent in the direction of the current from those cells, then reversed), and reading from the first fling of the galvanometer No. 2 by the aid of a reading telescope.

The time changes in the resistance of the diffusing solutions ascertained by this first fling method, are shown in the curve of resistance, fig. No. 1, which is constructed from the average of a series of six carefully repeated experiments, each extending over the tidal period of six hours. The highest resistance of the cells at the commencement was found to be 243 ohms, gradually reducing with a steady curve to 12 ohms at the termination of each experiment. This resistance curve, fig. No. 1, is the result of above 4,300 observations. This first fling method with a Daniell's element in circuit was used only for taking the rapidly changing resistance of the cells.

General Remarks.

A circumstance of interest in connexion with this research is the change of electro-chemical position which not unfrequently happened.

Another noticeable feature was the electro-chemical position maintained by the wrought iron bar (covered with its blue magnetic oxide) immersed in the sea water, this bar being in the negative position throughout; repeated experiments confirmed this.

Also when the bars were removed from the solutions at the close of an experiment, the different manner in which the metals had been acted upon in the respective cells was decidedly noticeable.

From an examination of the results (taking the highest and the average E.M.F.), it will be seen in what comparative manner the metals arranged themselves under the conditions of the experiment.

Table B.—The Electromotive Force, &c., during Diffusion of Sea Water and Distilled Water acting on the same Metal over Tidal Periods of Six Hours.

Time from commencement of experiment.		Time changes of resistance in the cells in ohms.	Wrought iron rolled bars (bright).		Wrought iron hammered bars (bright).		"Soft" Bessemer steel (bright).		"Hard" Bessemer steel (bright).		"Soft" Siemens-Martin steel (bright).		"Hard" Siemens-Martin steel (bright).	
hr.	min.		Electro-chemical position of the wrought iron rolled bar in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the wrought iron hammered bar in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the "soft" Bessemer steel in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the "hard" Bessemer steel in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the "soft" Siemens-Martin steel in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the "hard" Siemens-Martin steel in the sea water compartment.	Electro-motive force in volts.
0	0	243	P	0·015	N	0·016	P	0·040	P	0·080	P	0·059	P	0·003
0	15	102	P	0·095	P	0·021	P	0·064	P	0·114	P	0·115	P	0·130
0	30	51	P	0·092	P	0·036	P	0·049	P	0·119	P	0·075	P	0·121
0	45	32	P	0·085	P	0·035	P	0·043	P	0·132	P	0·064	P	0·109
1	0	25	P	0·078	P	0·030	P	0·037	P	0·135	P	0·052	P	0·098
1	15	20	P	0·069	P	0·027	P	0·030	P	0·133	P	0·046	P	0·085
1	30	17	P	0·063	P	0·023	P	0·026	P	0·130	P	0·040	P	0·074
1	45	17	P	0·058	P	0·019	P	0·021	P	0·127	P	0·034	P	0·068
2	0	16	P	0·054	P	0·016	P	0·016	P	0·122	P	0·029	P	0·061
2	15	15	P	0·050	P	0·013	P	0·014	P	0·117	P	0·026	P	0·055
2	30	14	P	0·045	P	0·010	P	0·012	P	0·111	P	0·020	P	0·051
2	45	13	P	0·041	P	0·009	P	0·009	P	0·106	P	0·017	P	0·046
3	0	13	P	0·036	P	0·007	P	0·006	P	0·100	P	0·014	P	0·040
3	30	13	P	0·029	P	0·005	P	0·002	P	0·089	P	0·012	P	0·032
4	0	12	P	0·023	P	0·004	N	0·002	P	0·079	P	0·010	P	0·027
4	30	12	P	0·018	P	0·004	N	0·004	P	0·068	P	0·009	P	0·019
5	0	12	P	0·014	P	0·004	N	0·006	P	0·060	P	0·004	P	0·016
5	30	12	P	0·011	P	0·004	N	0·009	P	0·051	P	0·002	P	0·014
6	0	12	P	0·009	P	0·006	N	0·011	P	0·045	Zero	no E.M.F.	P	0·013
			Highest E.M.F. attained, 0·095 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·054 of a volt.		Highest E.M.F. attained, 0·036 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·017 of a volt.		Highest E.M.F. attained, 0·064 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·024 of a volt.		Highest E.M.F. attained, 0·135 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·110 of a volt.		Highest E.M.F. attained, 0·115 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·038 of a volt.		Highest E.M.F. attained, 0·130 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·066 of a volt.	

N.B.—The averages in the above Table B, were obtained from the 91 observations previously alluded to.

Time from commencement of experiment.	Time changes of resistance in the cells in ohms.	"Soft" cast steel (bright).		"Hard" cast steel (bright).		Cast metal, No. 1 (bright).		Cast metal, No. 2 (bright).		Cast metal, No. 1 (in the rough).		Wrought iron (covered with its own blue magnetic oxide).	
		Electro-chemical position of the "soft" cast steel in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the "hard" cast steel in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the cast metal No. 1 in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the cast metal No. 2 in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the cast metal No. 1 in the sea water compartment.	Electro-motive force in volts.	Electro-chemical position of the wrought iron in the sea water compartment.	Electro-motive force in volts.
hr. min.													
0 0	243	P	0·073	P	0·020	Zero	no E.M.F.	Zero	no E.M.F.	P	0·075	N	0·059
0 15	102	P	0·120	P	0·083	P	0·076	P	0·043	P	0·076	N	0·043
0 30	51	P	0·057	P	0·087	P	0·058	P	0·025	P	0·045	N	0·038
0 45	32	P	0·043	P	0·081	P	0·047	P	0·016	P	0·042	N	0·035
1 0	25	P	0·032	P	0·072	P	0·040	P	0·013	P	0·039	N	0·034
1 15	20	P	0·026	P	0·065	P	0·034	P	0·011	P	0·036	N	0·031
1 30	17	P	0·021	P	0·058	P	0·030	P	0·008	P	0·034	N	0·028
1 45	17	P	0·016	P	0·052	P	0·027	P	0·005	P	0·033	N	0·027
2 0	16	P	0·013	P	0·047	P	0·022	P	0·003	P	0·030	N	0·026
2 15	15	P	0·011	P	0·041	P	0·016	P	0·003	P	0·030	N	0·025
2 30	14	P	0·009	P	0·037	P	0·014	P	0·002	P	0·029	N	0·023
2 45	13	P	0·005	P	0·031	P	0·012	P	0·001	P	0·028	N	0·021
3 0	13	P	0·004	P	0·027	P	0·010	P	0·001	P	0·027	N	0·020
3 30	13	N	0·001	P	0·020	P	0·005	N	0·001	P	0·026	N	0·017
4 0	12	N	0·003	P	0·012	P	0·003	N	0·001	P	0·023	N	0·018
4 30	12	N	0·004	P	0·010	Zero	no E.M.F.	N	0·002	P	0·022	N	0·018
5 0	12	N	0·004	P	0·007	N	0·002	N	0·003	P	0·019	N	0·017
5 30	12	N	0·003	P	0·004	N	0·004	N	0·001	P	0·018	N	0·017
6 0	12	N	0·002	P	0·002	N	0·004	N	0·0004	P	0·018	N	0·017
		Highest E.M.F. attained, 0·120 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·026 of a volt.		Highest E.M.F. attained 0·087 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·047 of a volt.		Highest E.M.F. attained, 0·076 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·027 of a volt.		Highest E.M.F. attained, 0·043 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·009 of a volt.		Highest E.M.F. attained, 0 076 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·035 of a volt.		Highest E.M.F. attained, 0·059 of a volt. Average E.M.F. for the whole tidal period of six hours, 0·028 of a volt.	

N.B.—The averages in the above Table were obtained from the 91 observations previously alluded to.

Twelve accurate curves of the electromotive force (each the result of ninety-one observations) were obtained, showing the effect of this tidal action on the various groups of metals under observation. The general contour and character of these affords interesting information respecting such action on the different metals employed. For brevity, however, the results are abridged and summarised in one Table, B.

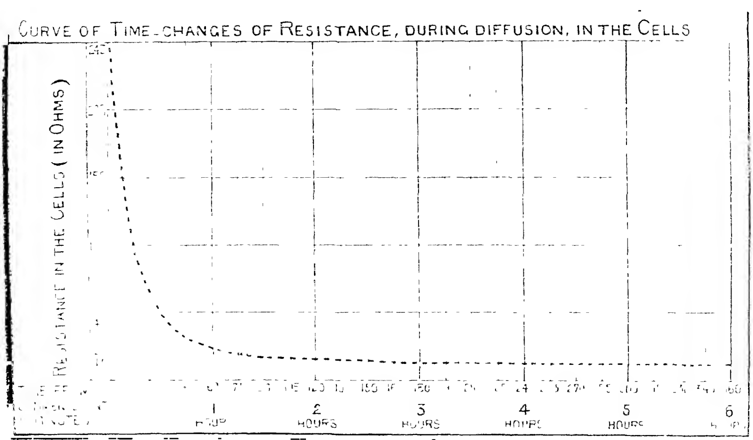
The highest E.M.F. was rapidly reached, and generally from this point to the end of a tidal period of six hours, a regular reduction of E.M.F. ensued. In every instance the greatest electromotive force was observed from the tidal action on the "hard" steels.

The "soft" steels affording on the averages less E.M.F. than the rolled wrought iron, but this group of steels generally gave a higher E.M.F. near the commencement of each experiment.

The rolled wrought iron gave more E.M.F. than the hammered wrought iron, which latter, together with cast metal No. 2, gave the least E.M.F. in these observations.

The preceding results give a quantitative measurement of the electromotive force under the conditions stated, and hence afford an indication of the extent of similar action on structural ironwork in tidal rivers during diffusion between the surface and lower waters, the electromotive force observed being not only appreciable, but in many instances very considerable, reaching not unfrequently *the one-tenth to the one-seventh of a volt.*

Fig. 1.



This destructive action would appear to be exerted most extensively on the lower portion of iron or steel vessels, metallic structures, &c.,

because such portion is shown by this research to be in the electro-positive position during certain conditions of a tidal stream.

It should be pointed out that similar conditions of galvanic action obtain in all our iron structures in tidal estuaries or rivers, the action of the salt and fresh water in course of diffusion constituting a source of galvanic disintegration independent of any difference in composition of the metals. It should also be observed that in circumstances where the electromotive force arising from causes here pointed out acts in concert with any E.M.F. from differences of composition of the metals employed in structures, a very considerable total electrolytic disintegration is likely to ensue. From data kindly furnished to the author by Dr. H. Clifton Sorby, F.R.S., an indication is afforded of the nature of the changing composition of the waters of tidal estuaries at various places and depths. The Table B of electromotive force, &c., together with the diffusion resistance curve (fig. No. 1), afford some index of the changing E.M.F. arising from such tidal difference of potential.

In approaching the subject in the manner stated in this memoir, the author trusts he has been able to afford some indication of the extent of the electromotive force from the action of tidal streams on the various metals experimented upon.

III. "On Unequal Electric Conduction-Resistance at Cathodes." By G. Gore, F.R.S., LL.D. Received April 30, 1884.

During some experiments which I have been making on the unequal resistance to the deposition of a metal upon cathodes of different metals in the same solution by the same current (see "Some New Phenomena of Electrolysis"), I have been led to investigate the resistance of cathodes of different metals to the passage of the current into them.

I have found that by taking a good conducting electrolyte, immersing in it a positive sheet of zinc, and a smaller negative one of another metal, connecting the plates with a galvanometer of low resistance, reducing all the other resistances in the circuit to the minimum except that of the negative plate; then making a series of measurements of strengths of current of different couples formed by the zinc and about twelve other metals, during removal of polarisation by stirring the liquid; also making another series of measurements of the electromotive forces of the same couples during stirring; calculating from these data the total resistance in each case, then deducting the portion of resistance due to the galvanometer, also that due to the liquid itself, and to opposing contact-potential, and

thermo-electric and voltaic action at the cathode and external junction, very different amounts of resistance, large in some cases, remain, and are exercised by different metals as cathodes, and those differences of resistance are only to a small extent due to heat and current absorbed in liberating hydrogen, and can only in a few cases be partly accounted for by chemical action, films, or absorption of gases at the cathode.

I am now investigating the nature of this resistance, and the relations of the resistance to various circumstances.

May 15, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Some Experiments on Metallic Reflection. No. V. On the Amount of Light reflected by Metallic Surfaces. III." By Sir JOHN CONROY, Bart., M.A. Communicated by Professor G. G. STOKES, Sec. R.S. Received May 6, 1884.

Professor Stokes recently communicated to the Royal Society ("Proc. Roy. Soc.," vol. 36, p. 187) an account of some determinations I had made of the amount of light reflected by steel and speculum metal mirrors when polarised light was incident upon them.

I have repeated these experiments with films of silver chemically deposited on glass, as such films approximate more closely to theoretically perfect metallic surfaces than any metallic mirror, however carefully polished, and also because very different statements have been made as to the reflective power of such films; one observer having said that silvered glass mirrors reflect about 90 per cent. of the light incident upon them, whilst another made their reflective power only equal to that of speculum metal.

I had hoped to have made some observations of the amount of light reflected by films of different thicknesses, which I had proposed to obtain in the same way as those used in the experiments of which an account was given in the "Proc. Roy. Soc.," vol. 31, p. 486, but after numerous trials, extending over a period of nearly two months, failed to obtain any suitable films.

Although the same process was used (Martins, "Ann. de Chim.," 4th series, vol. xv, p. 94) the silvered surfaces were never perfectly bright when removed from the solution, as had been the case with those prepared for the experiments already referred to. A few minutes' rubbing with a chamois leather was sufficient to render the mirrors perfectly bright, but the thin deposits of silver obtained by removing the glass plates from the solution before the action is at an end, are not sufficiently coherent to withstand the necessary amount of friction.

The first set of photometric determinations were made with a silver film deposited on a flat and well-polished glass plate 76·5 millims. long and 51 millims. wide; after being rubbed with a piece of chamois leather the surface was perfectly bright, and when compared with the speculum metal mirror in the way suggested by Professor Stokes ("Proc. Roy. Soc.," vol. 35, p. 36) the silvered surface appeared slightly the best of the two.

The glass plate was weighed before and after being coated with silver, and the weight of the film was found to be 0·0035 grm.; assuming the density of the silver to be 10·62, that being the value for silver finely divided by precipitation given in "Watts's Dictionary," vol. v, p. 277, the thickness of the film calculated from the area and weight was 0·00008447 millim., or about the same as the thickest of the films used in the experiments already referred to.

The film appeared opaque by ordinary daylight, but when examined with sunlight was seen to be slightly transparent and of a deep blue colour.

The photometrical determinations were made in exactly the same way as those with the speculum metal and steel mirrors ("Proc. Roy. Soc.," vol. 36, p. 187), and the observations were about as concordant as those contained in the Tables I and II of the paper giving an account of the experiments.

Two complete series of observations were made with light polarised in, and perpendicularly to, the plane of incidence, and the results are given in Tables I and II.

The angles of incidence are given in the first column, the percentage amount of light reflected in the second and third, the means of the two sets of observations in the fourth, and the amount of light which ought to have been reflected according to Cauchy's formulæ in the fifth.

Table I.—Silver Film, with Light polarised in the Plane of Incidence.

Angle of incidence.	Observed.			Calculated.
	A.	B.	Mean.	
30°	96·74	98·39	97·56	96·66
40	97·06	97·13	97·09	97·01
50	98·35	99·67	99·01	97·45
60	97·06	98·40	97·73	98·02
65	100·0	99·04	99·52	98·29
70	99·02	98·41	98·71	98·61
75	99·02	99·05	99·03	98·94

Table II.—Silver Film, with Light polarised perpendicularly to the Plane of Incidence.

Angle of incidence.	Observed.				Calculated.
	A.	B.	C.	Mean.	
30°	89·25	86·30	..	87·77	95·74
40	90·69	87·02	..	88·85	95·30
50	89·31	87·09	..	88·20	94·66
60	85·10	86·41	..	85·75	93·75
65	86·09	85·0	..	85·54	93·22
70	86·33	86·50	83·85	85·56	92·73
75	83·91	87·55	86·17	85·88	92·50

The principal incidences and the principal azimuths were determined in the manner described in "Proc. Roy. Soc.," vol. 31, p. 486, and vol. 35, p. 32, and the means of two sets of eight observations each are given below.

The values of the principal azimuths are higher than any obtained before in the course of these experiments, whilst those of the principal incidences are nearly the same as those obtained with the silver plate polished with rouge ("Proc. Roy. Soc.," vol. 31, p. 493), but considerably in excess of the determinations previously made with silver films.

Table III.

	Principal incidence.		Principal azimuth.
	75° 38′	......	44° 07′
	75 36	......	43 40
Mean	75 37	......	43 53

The calculated and observed values for the light polarised in the plane of incidence agree very fairly, the calculated values being slightly the lowest.

For light polarised perpendicularly to the plane there is considerable difference between the two sets of numbers, the calculated values being considerably the highest.

As has already been stated the silver film was, to some extent at least, transparent, and it was found that when a Nicol was held between the eye and the silvered glass, and sunlight was incident obliquely upon the film, the brightness and colour of the transmitted light varied with the position of the Nicol; the image of the sun being brightest when the short diagonal of the Nicol was in the plane of incidence, and darkest, and of a deep blue colour, when the long diagonal was in that plane. Hence it would appear that at oblique incidences light which is polarised perpendicularly to the plane of incidence penetrates to a greater depth in the film than that polarised in the plane, a result that is in accordance with the conclusion drawn from the experiments with silver films already referred to, and one that may account for the difference in the observed and calculated intensities of light polarised perpendicularly to the plane of incidence reflected by the silver film.*

In order to ascertain whether the difference between the observed and calculated results was really due to this cause or not, a thicker film was prepared by depositing a second coating of silver on a freshly-prepared film.

The same glass plate was used; the silver weighed 0·0072 grm., and

Table IV.—Double Silver Film, with Light polarised in the Plane of Incidence.

Angle of incidence.	Observed.			Calculated.
	A.	B.	Mean.	
30°	97·24	97·39	97·31	97·04
40	98·27	98·87	98·57	97·35
50	98·62	101·10	99·86	97·74
60	98·97	99·62	99·29	98·22
65	100·0	99·25	99·62	98·45
70	100·0	100·0	100·0	98·79
75	99·31	99·62	99·44	99·06

* Quincke, "Pogg. Ann.," vol. cxxix, p. 177, is of opinion that light polarised in and perpendicularly to the plane of incidence penetrates to an equal depth, but that the former is more rapidly diminished in intensity.

its thickness was therefore 0·0001737 millim., or as nearly as possible double that of the single film.

The thick film was not absolutely opaque, as the disk of the sun on a clear day could just be seen through it, but it transmitted much less light than the film previously used.

Tables IV and V give the results of two series of observations made with it, and also the theoretical amount of light which should have been reflected, calculated from the values of the principal incidence and principal azimuth given in Table VI.

Table V.—Double Silver Film, with Light polarised perpendicularly to the Plane of Incidence.

Angle of incidence.	Observed.			Calculated.
	A.	B.	Mean.	
30°	98·77 100·60	100·40	99·92	96·21
40	97·60	97·50	97·55	95·82
50	98·20	96·28	97·24	95·24
60	97·62	95·67	96·64	94·43
65	95·88	95·68	95·78	93·94
70	94·20	93·11	93·66	93·48
75	94·03	93·77	93·90	93·26

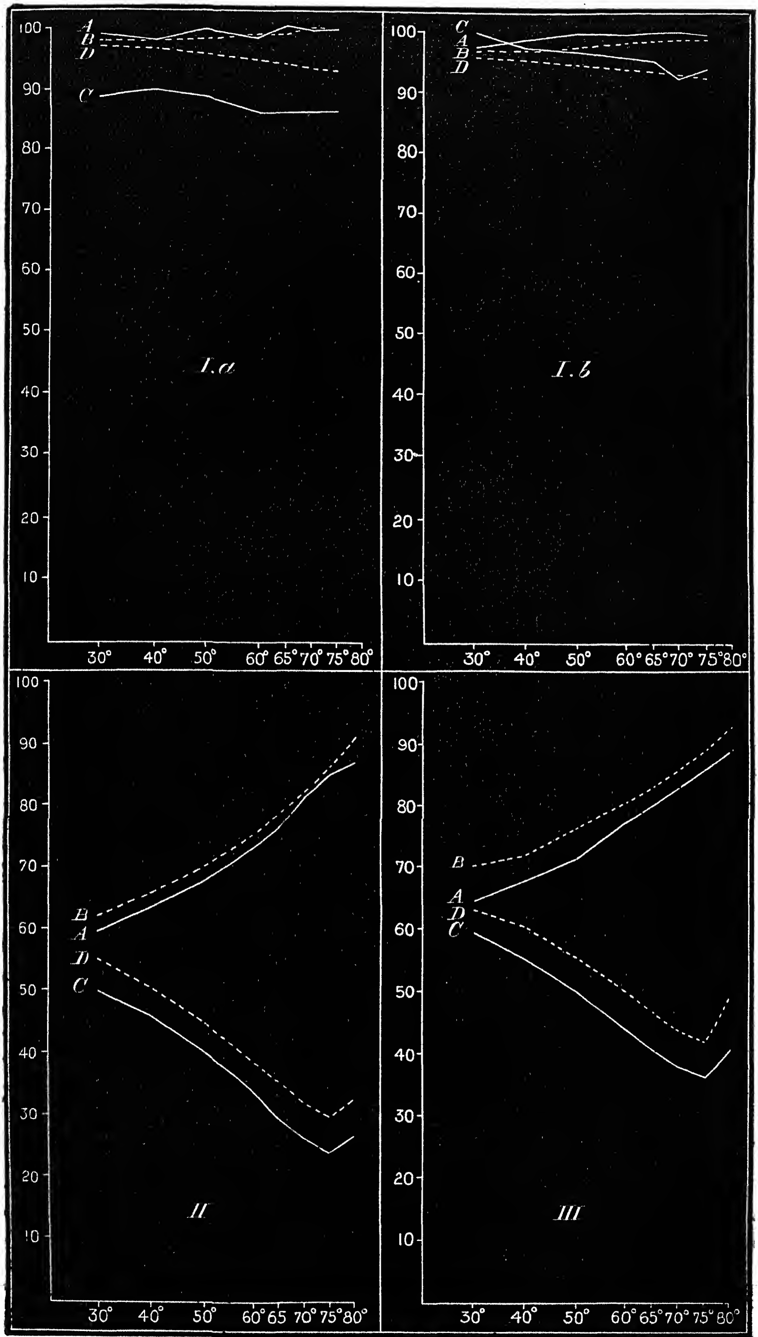
Table VI.

	Principal incidence.		Principal azimuth.
	75° 50′	......	43° 52′
	75 45	......	44 07
Mean....	75 47	......	44 0

The values of the principal incidence and azimuth are slightly higher than those obtained with the thinner film, and therefore the percentage amount of light which, according to theory, should be reflected by the silver, is also higher.

The tables show that both for light polarised in and perpendicularly to the plane of incidence the observed intensity exceeds the calculated intensity, in the former case by about 1, and in the latter by about 2 per cent., except at incidences of 30° with light polarised in the plane, and 70° and 75° for light polarised perpendicularly to the plane, for which angles the observed and calculated intensities agree closely.

Curves were drawn to show the calculated and observed intensities,


I. a
I. b
II
III
A
B
C
D
100
90
80
70
60
50
40
30
20
10
30°
40°
50°
60°
65°
70°
75°
80°

the angles of incidence being taken as abscissæ, and the intensities as ordinates; those representing the calculated intensities being drawn with dotted lines.

The inequalities in that for the observed value with light polarised in the plane of incidence, and the thin silver film (Fig. I*a*) would probably have disappeared if a larger number of observations had been made, the curve for the calculated value, however, corresponds very nearly with the line which would have been obtained had a smooth curve been drawn between the points given by observation.

Fig. I*b* gives the results with the thick silver film.

Figs. II and III represent the observations made with the steel* and speculum metal mirrors. (" Proc. Roy. Soc.," vol. 36, p. 187.)

These diagrams appear to confirm the general conclusion arrived at in the former paper, that although the received formulæ for metallic reflection are approximately correct, they are not a complete expression of the facts of the case.

EXPLANATION OF DIAGRAMS.

I*a*. Amount of light reflected by silver film.
I*b*. Amount of light reflected by double silver film.
II. Amount of light reflected by steel mirror.
III. Amount of light reflected by speculum metal mirror.

A. Light polarised in the plane of incidence. Observed.
B. " " " Calculated.
C. " perpendicularly to the plane of incidence. Observed.
D. " " " " Calculated.

II. "On the Influence of Coal-Dust in Colliery Explosions. No. V." By W. Galloway. Communicated by R. H. Scott, F.R.S. Received May 8, 1884.

At the beginning of the first paper on this subject, which I had the honour of reading before the Fellows of the Royal Society now somewhat more than eight years ago (" Proc. Roy. Soc.," vol. 24, p. 354), I gave a short account of what appeared to me to be a rational mode of explaining the occurrence of all great explosions in dry and dusty collieries; and since then I have had opportunities of studying several remarkable instances of this class of phenomena, with the result that I am now more than ever satisfied with the correctness of the views which I then expressed. It is true, as some subsequent writers, amongst whom I may name Sir Frederick Abel, F.R.S., have observed, that coal-dust had been previously recognised as a factor in

* The value of I^2 for steel at an angle of 50° is 46° 14′, and not 42° 09′, as given in the table on p. 196, an error for which, I regret to say, I am responsible.

colliery explosions. I think I may safely claim, however, that no earlier author had gone the length of crediting it with the *rôle* of principal agent, and relegating fire-damp to a secondary position.

It is also admitted, I believe, by everyone familiar with the subject, that my experiments with mixtures of coal-dust and air containing a small proportion of fire-damp were original. Similar experiments were subsequently made by members of the North of England Institute of Mining and Mechanical Engineers,* by a committee of the Chesterfield Institute of Engineers,† by Professor Abel on behalf of the Home Office and the Royal Commission on Accidents in Mines,‡ and by others in this country,§ by MM. Mallard and Le Chatelier for the Commission du Grisou in France,‖ and by others on the Continent, all of which led to the same conclusion, namely, that air containing too small a proportion of fire-damp to render it inflammable at ordinary pressure and temperature becomes so when coal-dust is added to it. Differences of opinion were expressed as to the actual proportion of fire-damp, the comparative fineness of the coal-dust, and the quality of the coal necessary to the attainment of this result, but the general conclusion, in every case, was the one I have stated above.

In my first paper, already referred to, I had said: "If it could be shown, therefore, that a mixture of air and coal-dust is inflammable at ordinary pressure and temperature, there could be no difficulty in accounting for the extent and violence of many explosions which have occurred in mines in which no large accumulations of fire-damp were known to exist," and, immediately following these words, I gave what appears to me to be a new hypothesis regarding the mode of occurrence of great colliery explosions.

My reasons for thinking it necessary to show that a mixture of air and coal-dust alone is inflammable were, first, that after some

* "Trans. N. of E. Inst. of Mining and Mechanical Engineers," vol. xxviii, p. 85.

† "Trans. Chesterfield and Derbyshire Inst. Mining, &c., Engineers," vol. x, Parts I and II.

‡ (*a*) Report on the Results of Experiments made with samples of Dust collected at Seaham Colliery in compliance with the request of the Secretary of State for the Home Department, conveyed by a letter, dated November 4th, 1880. By F. A. Abel, C.B., F.R.S., President of the Institute of Chemistry, Chemist to the War Department, &c.

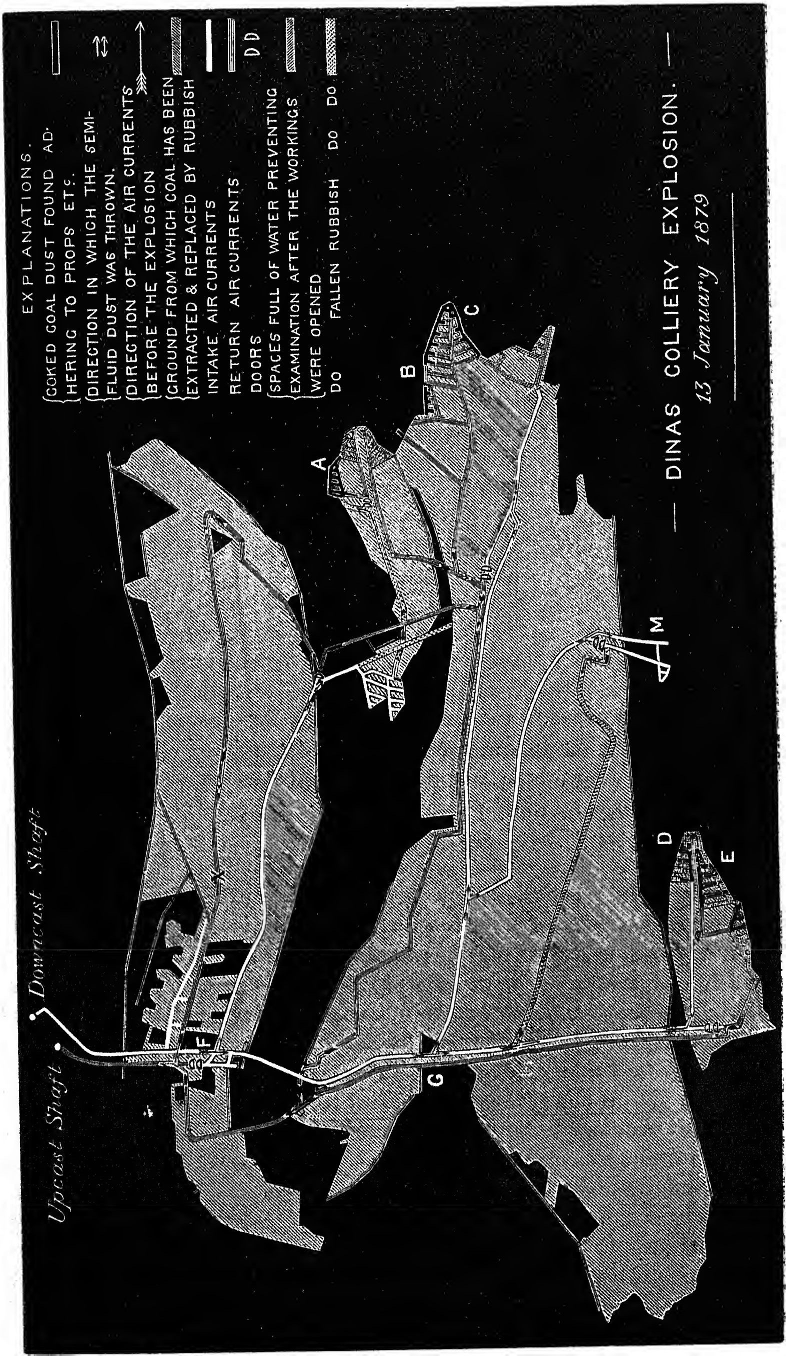
(*b*) "Some of the Dangerous Properties of Dusts." By F. A. Abel, C.B., F.R.S. A lecture delivered at the Royal Institution of Great Britain, on Friday, 28th April, 1882.

§ "The Explosive Properties of Coal-dust, Coal-gas, and Atmospheric Air, with special reference to Mines." By C. E. Jones, F.R.H.S., Chesterfield. Read before the Manchester District Institution of Gas Engineers on the 24th of February, 1883.

‖ "Annales des Mines," 1re livraison, 1882.

great explosions it was found that the flame had passed through very long galleries, containing presumably nothing but pure air, and of course dry coal-dust in a state of greater or less purity; and, secondly, it was impossible to account for certain other explosions, except on the supposition that they had been originated by the firing of a shot in pure air in galleries containing dry coal-dust as in the last case. To have proved that a mixture of air, coal-dust, and fire-damp is inflammable did not appear to me fully to meet the case, and it was for this reason that I made further experiments with the help of a grant made to me by the Lords of Committee of Council on Education at the recommendation of this Society. The results have been described in some of the former papers of this series. In making these experiments, and in drawing certain conclusions from them, all favourable to the hypothesis referred to, I was simply carrying out the details of the work then begun, and nothing more.

In former papers I referred to several great explosions which had come under my own immediate observation. In particular I had made a very careful and complete examination of Penygraig Colliery after the explosion there in December, 1880 ("Proc. Roy. Soc.," vol. 32, p. 454), when I found that the flame had penetrated into every working place in the mine. The plan which accompanies No. III paper shows that all the working places were ventilated by what was, practically, a single current of air. It was, therefore, open to those who attribute every great explosion to the occurrence of a sudden outburst of fire-damp, and, as the annals of mining show, they constituted a very large majority before the appearance of my first paper on coal-dust, to say that this explosion was due to the same cause. For this reason I have paid particular attention to the phenomena due to the explosion which occurred at Dinas Colliery on the 13th of January, 1879. I do not propose to enter into the minute details of this case, as I should to a large extent simply be repeating what I stated about Penygraig explosion, but will confine myself to those which are necessary or new. I had frequently inspected the workings before the explosion, and I have done so at intervals of one month or less since then, so that I have been intimately acquainted with all the conditions of the mine for many years. I know also that no sudden outburst of fire-damp has ever been known to take place in it. The workings were naturally very dry, the temperature ranging from 75° to 82° F., and the floor was covered with coal-dust. Shot firing was carried on by night when the explosion happened. The damage done by the explosion was very great, the workings being wrecked to such an extent as to lead to their temporary abandonment. They were re-opened after a large expenditure of time and labour, and it was only towards the end of last year that I was able to inspect the


EXPLANATIONS.
COKED COAL DUST FOUND ADHERING TO PROPS ETC.
DIRECTION IN WHICH THE SEMI-FLUID DUST WAS THROWN.
DIRECTION OF THE AIR CURRENTS BEFORE THE EXPLOSION
GROUND FROM WHICH COAL HAS BEEN EXTRACTED & REPLACED BY RUBBISH
INTAKE AIR CURRENTS
RETURN AIR CURRENTS
DOORS
D D
SPACES FULL OF WATER PREVENTING EXAMINATION AFTER THE WORKINGS WERE OPENED
DO FALLEN RUBBISH DO DO
DINAS COLLIERY EXPLOSION.
13 January 1879
Upcast Shaft
Downcast Shaft
A
B
C
D
E
F
G
M
X

working places A, B, C, shown in the accompanying plan, and early in the present year that I could get into those marked D, E. With the exception of some burnt hay or dried grass which I found at the point X in one of the return air-ways, I saw no traces of burning nor deposits of coked coal-dust in any of the main roadways, but I found well-marked deposits of coked coal-dust in all the working places in both districts of workings as far as I was able to penetrate. The plan shows that the current of fresh air which came down the downcast shaft was split up into three separate currents at the points K and L. The districts A, B, C, and D, E, were thus ventilated quite independently of each other, and thus it was impossible for any outburst of fire-damp which might take place in one of them to affect the quality of the air in the other.

We are thus compelled to fall back upon some other mode of explanation in this case, and I now submit that in the present, and in my previous papers, I have brought forward sufficient evidence to show that the coal-dust hypothesis is the only tenable one. If it be admitted, however, that this hypothesis is applicable to Dinas explosion, the conclusion is inevitable that, *cæteris paribus*, it is equally applicable to every case of the same kind that has ever occurred.

III. "Observations on the Ingesta and Egesta of Mr. Edward Payson Weston during his Walk of 5,000 miles in 100 days. By A. WYNTER BLYTH, Medical Officer of Health for St. Marylebone. Communicated by B. W. RICHARDSON, M.D., F.R.S. Received April 30, 1884.

On the 15th day of March, 1884, Mr. Edward P. Weston finished successfully a pedestrian feat, which consisted in walking 50 miles a day, Sundays excepted, until he had traversed 5,000 miles.

The last 300 miles were walked on a level track in the Victoria Hall; and frequent observations of the pedestrian's pulse and respiration, &c., were made during six days by Mr. Green, M.R.C.S., acting under the instructions of a Committee, the active members of whom were Dr. B. W. Richardson and Dr. Ridge. Mr. Green also made some volumetric determinations of urea for the purposes of his clinical report; and he measured the urine, preserved the fæces, and weighed or measured all the food, whether liquid or solid.

Day by day I received the urine for detailed analysis, and at the termination of the walk I also received a jar containing the fæces collected during the five days ending at midnight, Saturday, 15th March.

Table I.—Giving the Details of the various Foods consumed by Mr. Weston during each of the Six Days he was under scientific Observation.

	Bread-stuffs and starchy foods.								Fat.	Nitrogenous foods.							Fruit.	Liquids.		
	Oatmeal taken as porridge.	Dry toast.	Bread.	Muffins.	Sponge cake.	Tapioca pudding.	Potatoes.	Cabbage.	Butter.	Fish (cod).	Roast beef.	Roast mutton.	Eggs.	Meat wafers.	Mutton broth.	Milk.	Figs.	Zoedone, ginger ale, &c.	Tea.	Coffee.
	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	No.	grms.	grms.	grms.	grms.	grms.	grms.	grms.
Monday—																				
Breakfast	75·46	56·67	...	...	...	...	...	...	56·7	...	...	...	3							
Lunch and dinner	...	42·50	293·4	...	...	340·2	113·4	141·7	...	...	311·8									
Supper	...	...	...	...	56·69	226·8	...	...	...	...	...	...	...	...	453·4	283·5	99·2	481·9	453·5	1162·3
Tuesday—																				
Breakfast	81·62	...	...	85·0	...	...	...	...	36·84	269·3										
Lunch and dinner	...	56·67	193·4	...	...	340·2	113·4	141·7	...	...	...	184·2								
Supper	...	56·67	...	...	56·69	226·8	...	...	...	...	...	...	...	...	340·0	...	63·76	566·9	425·2	652·0
Wednesday—																				
Breakfast	125·85	42·50	...	121·9	...	...	...	...	59·52	226·8	...	...	2							
Lunch and dinner	...	56·67	317·78	...	...	198·4	83·0	113·4	...	...	184·2									
Supper	...	56·67	...	...	56·69	283·5	...	...	...	...	...	...	...	...	425·2	141·7	85·04	566·9	680·4	1020·1
Thursday—																				
Breakfast	106·87	...	...	...	...	...	...	...	56·69	226·8										
Lunch and dinner	...	56·67	280·90	...	...	255·14	113·4	113·4	...	...	212·5	...	...	28·35						
Supper	...	...	...	...	56·69	581·16	...	...	...	...	...	...	...	...	425·2	...	85·04	108·7	425·2	850·4
Friday—																				
Breakfast	106·87	56·67	...	97·5	...	...	...	...	77·94	226 8										
Lunch and dinner	...	63·75	433·1	...	...	340·20	97·2	113·4	...	...	212·5									
Supper	...	...	...	...	56·69	269·30	...	...	...	...	...	...	...	...	340·0	...	70·8	1133·9	...	1644·3
Saturday—																				
Breakfast	119·34	42·51	...	...	...	...	...	...	45·62	...	...	...	4							
Lunch and dinner	...	56·67	293·4	...	...	333·0	113·4	141·7	...	...	...	191·3								
Supper	...	56·67	...	...	...	...	...	...	...	...	...	...	...	...	425·2	...	...	1275·6	510·3	680·4
	616·01	701 29	1811·98	304·4	283·45	3394·7	633·8	765·3	333·31	949·7	921·0	375·5	9	28·35	2409·0	425·2	403·8	4133·9	2494·6	5009·5

Dr. Richardson has published elsewhere* an account of the results of the general clinical observations, and it suffices to remark, that the pedestrian during the whole time enjoyed the most robust health, and at the termination of the walk was apparently in no way the worse for the continuous exertion of so many months.

My own part in the observations on Weston has been the analysis of the egesta and the calculation of the food equivalents. The latter have been, for the most part, based upon analyses in my own work on "Food," supplemented by the mean numbers given in König's "Nahrungsmittel," and in two instances by analyses of the actual foods consumed.†

The Food taken. Ingesta.

The bulk of the food was taken in three meals—breakfast, dinner, and supper. Carbohydrates were supplied chiefly through porridge, bread-stuffs, potatoes, and tapioca pudding; albuminoids through eggs, fish, lean beef or mutton; some fat was taken in the shape of butter, and there was a copious supply of liquids by means of mutton broth, tea, coffee, and aërated waters; it is also noticeable that each day a certain number of figs were eaten. Table I (p. 47) gives the amounts of the various constituents in grams and cubic centimetres.

In Table II the somewhat complicated diet is reduced to "food equivalents."

The last day, Saturday, was in some points not comparable with the others; besides which, we have the results of an analysis of the solid excreta‡ corresponding to the food of the five days, from Monday to Friday inclusive, so that it is best to take the average of the five days previous to Saturday, and this average may be compared with any standard diet which would keep a man of Weston's weight in health during ordinary labour.

* The "Asclepiad," No. 2, vol. i, p. 166, *et seq.*

† *E.g.*, the pudding (of which so much was taken) referred to in Table I as tapioca pudding I found to contain—

Water	31·00
Albuminoids	7·72
Carbohydrates	61·04
Fat	0·21
Ash	0·03
	100·00

The sponge cake was also analysed.

‡ The solid excreta of, say, Tuesday is supposed to belong to Monday's food, that of Wednesday to Tuesday, and so on; but as the urinary secretion is expelled more frequently, most of a day's urine belongs to the diet of that particular day, and in this paper is supposed to wholly belong to the same day.

Table II.—Reducing the various Foods tabulated in Table I to Food Equivalents.

	Water.	Albuminoids.	Carbohydrates.	Fat.	Ash.
	grms.	grms.	grms.	grms.	grms.
Monday	3889 ·2	226 ·8	779 ·6	45 ·35	22 ·68
Tuesday	3107 ·0	221 ·1	700 ·2	51 ·02	19 ·84
Wednesday	4144 ·5	243 ·8	785 ·3	93 ·55	31 ·17
Thursday	3696·6	246 ·6	887 ·2	45 ·35	19 ·84
Friday	4110 ·5	240 ·9	847 ·5	87 ·88	19 ·84
Saturday	3977 ·3	164 ·4	552 ·8	70 ·87	17 ·01
Total	22925 ·1	1343 ·6	4552 ·6	394·02	130 ·38
Mean of the six days	3820 ·8	223 ·4	758 ·7	65 ·67	21 ·73
Mean of five days, Saturday being excluded	3789 ·5	235 ·8	799 ·9	64 ·63	22 ·67

Table III.—Daily Average of Water-free Food taken by Mr. Weston as compared with a Standard Diet.

	Standard diet.	Average of Weston's food.
	grms.	grms.
Albuminoids	98 ·46	235 ·80
Carbohydrates	437 ·98	799 ·90
Fat	47 ·40	64 ·63
Mineral matters	24 ·37	22 ·67*

The albuminoids are therefore about 2·5 times that of an ordinary diet, and the carbohydrates and the fat nearly double.

The Urine.

The urine daily measured by Mr. Green, and forwarded to me, was always normal in colour, and contained neither albumin nor sugar.

Each sample was also tested for indigo by adding an equal bulk of strong hydrochloric acid and the proper quantity of bleaching powder, and then shaking up with chloroform; but in no instance could any evidence of indigo be obtained. By the time the urine reached me it had always undergone some ammoniacal decomposition, and any

* The mineral matter is much too low, as is proved by the fact that the mineral substances excreted were in excess of the calculated ingested mineral substances; the explanation of which is that I have had no weights given me of the salt taken at meals, nor have I sufficient data for the mineral constituents of the broth, tea, coffee, and liquids drank.

ordinary estimation of urea would have given low results; it therefore appeared better to estimate the total nitrogen, and with certain deductions described further on to calculate out the urea from the data thus obtained. The total nitrogen was estimated by the moist process of combustion, first proposed, I believe, by Vijeldahl ("Zeitsch. für Analytische Chemie," Heft 3, 1883), and recently submitted to a very exhaustive research as applied to urine by Dr. Petri and Th. Lehmann ("Zeitsch. für Physiologische Chemie," Band VIII, Heft 3, 1884). It is a very accurate method of determining the total nitrogen in urine, in milk, and probably in most organic liquids, and will, without doubt, be much used.

Two grms. of the urine were placed in a flask, and 20 cub. centims. of pure sulphuric acid added; heat was applied by means of a small flame for two or three hours, at the end of which time crystals of permanganate were added until the liquid was first decolorised, and then given a distinct dark pink or red tint. On now alkalising with pure soda, all the nitrogen present was distilled over as ammonia; the distillation being assisted by a current of hydrogen gas; the ammoniacal distillate was received in a known quantity of standard decinormal acid, and titrated back by decinormal soda, using as an indicator phenolphthaleïn.

The uric acid was determined in the usual way, viz., by concentrating half a litre of the filtered urine, strongly acidifying with hydrochloric acid, and, after twenty-four hours, collecting the crystals on a weighed filter.

The hippuric acid was estimated by adding some recently calcined magnesia to a litre of the urine, concentrating to a syrup, acidifying the syrup with hydrochloric acid, and extracting the acid liquid with ether.

The alkaloids (mostly kreatinine) were determined by acidifying with sulphuric acid, and then precipitating by phosphomolybdic acid; the precipitate was decomposed by baryta at a boiling temperature; the phosphomolybdate of baryta filtered off and well washed; the filtrate containing the urine alkaloids and baryta was saturated by carbon dioxide, boiled, concentrated, and filtered, the filtrate being evaporated to dryness and weighed; as there was still a trace of baryta, it was necessary to ignite, re-weigh, and to take the loss as representing the total alkaloids.

The urea, as before said, was not estimated directly, but was calculated from the total nitrogen after subtracting the nitrogen in the uric and hippuric acids and in the urine alkaloids; it may be a trifle over-estimated.

For the total solids 5 grms. of the urine were evaporated at 100° C. in a platinum dish, until the weight was practically constant. From a larger quantity concentrated to a syrup, the mineral matter was

Table IV.

Analyses of the Urine of Mr. P. Weston during the Six Days he was under Observation.

	Urea.	Uric acid.	Hippuric acid.	The urine alkaloids chiefly kreatinine.	Total nitrogen.	Phosphoric acid as phosphate.	Sulphuric acid (sulphates).	Chlorine.	Mineral matter.	Total solids.	Water.
	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.
Monday.....	66·55	·48	·83	3·22	27·30	4·83	6·10	23·00	70·84	125·50	2174·5
Tuesday	47·74	·50	·29	·65	19·00	5·97	5·37	3·37	27·32	95·54	1844·4
Wednesday..	53·93	·26	·14	·48	21·25	6·76	6·63	14·98	32·74	107·00	2033·0
Thursday....	63·39	·14	·24	·23	24·80	5·81	5·74	11·59	24·28	93·20	1746·8
Friday	63·44	·53	·37	·66	25·13	5·60	7·76	13·23	27·21	87·88	1802·1
Saturday....	56·05	·53	Undetermined	1·34	22·49	5·14	5·96	9·18	24·17	75·27	1454·7
Total	351·10	2·44	1·87	6·58	139·97	34·11	37·56	75·35	206·56	584·39	11055·5
Mean....	58·51	·40	·37	1·09	23·32	5·81	6·26	12·57	34·42	97·39	1842·5

determined in the usual way. The methods used for estimating the other constituents were ordinary methods, and need not be detailed. Table IV gives the total excretion of the various substances mentioned, by the kidney, during each of the six days.

Excretion of Sulphates and Organic Sulphur.

As a subsidiary research, the sulphates were precipitated from a measured sample of the urine by barium chloride, and after the precipitate had been separated, the organic sulphur compound was broken up by saturating the liquid with chlorine. By this means, a second precipitate of barium sulphate was thrown down, and in the following table is calculated into organic sulphur.

Table V.—Amount of Sulphur as Sulphates and as Organic Sulphur separated daily, and compared with the Quantities of certain Foods.

	Sulphur as sulphate.	Organic sulphur.	Total sulphur.	Bread-stuffs.	Albuminoids.
	grms.	grm.	grms.	grms.	grms.
Monday.....	2·79	·068	2·858	392·57	226·8
Tuesday	2·47	·485	2·955	396·74	221·1
Wednesday..	3·02	·565	3·585	595·52	243·8
Thursday ...	2·61	·304	2·914	137·57	246·6
Friday......	3·52	·362	3·882	651·03	240·9
Saturday ...	2·75	·631	3·331	449·25	164·4

It is possible to arrive at the probable origin of the sulphur both oxidised and unoxidised, by arranging the days of the week in order so that the day of most sulphur excretion is at the top, of least sulphur excretion at the bottom; and then to take from Table I the several foods, and arranging them in a similar order to see whether there is any coincidence.

The days in the order of sulphur excretion are Friday, Wednesday, Saturday, Tuesday, Thursday, and Monday.

Since albumin constantly contains sulphur, the sulphates might be governed by the total albuminoids, but a glance at Table V will show that the albumin sequence is Thursday, Wednesday, Friday, Monday, Tuesday, and Saturday, which has no relationship with the sulphur sequence. It may be similarly shown that for a like reason, the liquids, the cabbages, the oatmeal, and all other constituents save the bread-stuffs may be excluded.

The toast, the bread, and the muffins (when taken) for each day

added together, and then the days arranged in order, show a very remarkable coincidence as follows, and renders it in the highest degree probable that the greater or less excretion of sulphates depended in Weston's case on the amount of bread-stuffs consumed.

Ingestion of bread-stuffs.	Sulphur excretion.
Friday	Friday
Wednesday	Wednesday
Saturday	Saturday
Tuesday	Tuesday
Monday	Thursday
Thursday	Monday

Note on the Urine Alkaloids.

The alkaloids of the urine, especially kreatinine, have been variously considered as derived from metabolism of the living muscle, or from nitrogenous food. It is instructive to observe that the urine alkaloids in Weston's case were in excess on Monday, a day which followed twenty-four hours of complete rest, while during the rest of the week save Saturday, the amounts were rather below normal, the days arranged in series of most alkaloid down to least alkaloid are Monday, Saturday, Friday, Tuesday, Wednesday, Thursday, and this series shows no agreement with either the meat ingested or the broth. It would, therefore, seem that exertion, so far from increasing the excretion of the flesh bases, oxidises them up probably to urea.

Fæces.

The solid excreta for the five days, Tuesday to Saturday inclusive, were mixed as thoroughly as possible, and analysed as follows :—

A weighed portion was digested with strong alcohol to remove as much of the water as possible, the dehydrated residue was then exhausted in a Soxhlet's apparatus by ether; the alcoholic and ethereal extracts were united, dried, and weighed; the exhausted residue was also dried and weighed; the difference between the sum of these weights and the original was taken as representing the water. I may remark that I have always found the method detailed give more constant results than drying in the usual way.

The fat and cholesterin were extracted from a weighed portion by ether-alcohol, the volatile solvents being driven off, the extract was dried, redissolved by anhydrous ether-alcohol, and on evaporation weighed as fat and cholesterin; subsequently the cholesterin was extracted by saponifying the fat, and treating the soap with ether.

The fatty acids were extracted from the fæcal soaps by acidifying with hydrochloric acid a portion of the excreta already freed from fat by ether, and then extracting with the same solvent. Combustions were made for nitrogen, carbon, and hydrogen.

Undigested starchy matters were determined by conversion into sugar by boiling with a dilute acid and then titrating with Pavy's ammoniacal copper solution. The whole analysis is as follows :—

	The solid excreta (five days). grms.
Water	1695 ·29
Albuminoids	309 ·23
Starchy matters	47 ·96
Fat	7 ·76
Fatty acids	36 ·77
Cholesterin	33 ·35
Cellulose and insoluble matters	66 ·69
Ash	86 ·78
	2283 ·83

Summary of Results.

A good idea of the amount of food actually consumed, or used by the muscular mechanism, may be obtained by giving a sort of balance sheet between the food equivalents and the excreta, thus :—

	Ingesta during the five corresponding days. grms.	Egesta. grms.	Difference. grms.
Water	18,947 ·8	11,296 ·09	−7651 ·7
Fat	323 ·15	77 ·88	− 245 ·3
Carbohydrates	3,999 ·8	114 ·65	−3885 ·2

The albuminoids in the food during the five days in which the total egesta were collected, amounted to 1179·2 grms., representing 186·2 grms. of nitrogen; the total nitrogen eliminated through the kidneys during the same period was 117·5 grms., and through the bowels 48·8 grms.; this subtracted from the nitrogen of the food gives a difference of 19·9 grms.; hence although all the nitrogen excreted is accounted for as derived from the food, some seems to have been retained, and the nitrogenous equilibrium was not quite perfect.

K. Vierordt ("Grundriss der Physiologie des Menschen," 1877) calculated on the assumption that 1 grm. of carbon completely burnt is equal to 8080 heat-units, and 1 grm. of hydrogen to 34,460 heat-

units, that the diet of a certain man consisting of 120 grms. of albuminoids, 90 of fat, and 330 of carbohydrates was equal to 2,922,011 heat-units; subtracting the carbon and hydrogen excreted in the fæces and urine, the heat-units of which were equivalent to 457,882, there were left 2,464,129 heat-units as representing the carbon and hydrogen which may be considered to have undergone complete combustion. A similar calculation gives the mean daily numbers of Weston's heat-units as 4,690,183, or just about twice the amount that Vierordt calculated for a man doing ordinary work on the standard diet given above.

Assuming the usual formula to be correct, that a person walking on a level surface raises $\frac{1}{20}$ of his weight through the distance walked, then the work done by Weston daily in the Victoria Hall was equal to 2,462,071 kilogram-metres (793 foot-tons). It has usually been held that 1,552,795 kilogram-metres (500 foot-tons) daily was excessive labour, most ordinary work being little more than one-third of this.

Although for a limited time Weston himself, as well as others, has undergone more exertion, his feat is, I believe, the greatest recorded labour, if its continuity be considered, that any human being has ever undertaken without injury.

The Society adjourned over Ascension Day to Thursday, May 29th.

May 29, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Mr. Henry B. Medlicott (elected 1877) was admitted into the Society.

The following Papers were read:—

I. "The Conditions of Chemical Change in Gases: Hydrogen, Carbonic Oxide, and Oxygen." By HAROLD B. DIXON, M.A. Communicated by Professor A. W. WILLIAMSON, Foreign Sec. R.S. Received May 15, 1884.

(Abstract.)

Bunsen,* in 1852, exploded mixtures of carbonic oxide and electrolytic gas in different proportions in order to test the correctness of the "Law of Mass." According to this law the division of oxygen between two combustible gases depends both on the relative affinity of the two combustibles for oxygen, and on the quantities of them present. In 1857 Bunsen† published some further experiments on the same subject. He concluded from his results that the Law of Mass was modified in a particular way by the tendency of the atoms to form simple hydrates of carbonic acid; so that the ratio of carbonic oxide to hydrogen might be changed within certain limits without altering the proportion in which the oxygen divides itself; but, on still further changing the ratio of carbonic oxide to hydrogen, the proportion in which the oxygen divides itself changes *per saltum*. The ratio between the carbonic acid and the stream produced in the explosion might always be expressed, according to Bunsen, by small integers.

In 1874 E. v. Meyer‡ published experiments on the incomplete combustion of mixtures of carbonic oxide and hydrogen by oxygen

* "Ann. Chem. Pharm.," lxxxv, 137.
† Bunsen, "Gas Meth.," 1857.
‡ "Journ. Prakt. Chem." (ii), x, 273.

and by nitrous oxide. He concluded that the division of the oxygen between the combustible gases changes *per saltum*, and that the ratio between the carbonic acid and steam produced might always be expressed by whole numbers, but not always by small integers.

v. Meyer found that the presence of an inert gas, nitrogen, favoured the formation of carbonic acid. He also found that more steam was generally formed when the explosions were made in narrow tubes than when they were made in wide tubes.

In 1876 Horstmann* published a paper in a local journal at Heidelberg in which he showed that Bunsen's results were vitiated by his having exploded the gaseous mixtures in a eudiometer saturated with aqueous vapour. At the high temperature of the explosion, steam oxidises carbonic oxide, so that the proportion of carbonic acid found after the explosion partly depended upon the initial temperature of the eudiometer, and the quantity of steam consequently present.

Horstmann showed that there was no discontinuous alteration in the ratio of the carbonic acid to steam formed in the explosion either in presence of aqueous vapour or without.

In the same year, 1876, in ignorance of Horstmann's paper, I repeated Bunsen's experiments, and came to the same conclusion as Horstmann. I found that the aqueous vapour in the eudiometer reacted with the excess of carbonic oxide at the high temperature reached. I repeated the experiments, drying the eudiometer and the gases carefully before exploding. My results gave no evidence of any change *per saltum* in the division of the oxygen.

In testing the dried gases it was discovered that an electric spark does not ignite a dry mixture of carbonic oxide and oxygen.† A trace of aqueous vapour was found to render the mixture explosive, all other conditions being the same. A mixture of the gases imperfectly dried with freshly fused potash was found to be unaffected by a spark at pressures below 500 millims. At a pressure of 500 millims. the mixture was ignited by the spark and burnt *slowly*. When left in contact with anhydrous phosphoric acid, either over mercury or sealed up in tubes, a mixture of two volumes of carbonic oxide with one of oxygen may be subjected, under atmospheric pressure, to powerful sparks from a coil or Leyden jar without exploding or igniting. The addition of a trace of steam, of dry hydrogen, dry ether vapour, dry pentane vapour, dry sulphuretted hydrogen, or dry hydrochloric acid vapour, was found to render the mixture inflammable, all the other conditions being the same. Dr. Bötsch‡ has published a paper in which he denies the non-inflammability of dry

* "Verh. des Heidelb. Naturf. Med. Vereins," N.S., i, 3.

† "British Assoc. Report," 1880.

‡ "Liebig. Annalen," 1882.

carbonic oxide and oxygen; he considers that the inflammability of the mixture depends only upon pressure, and that in my experiments the mixture was tested under a less pressure when dry than when wet. This explanation of Dr. Bötsch's does not account for the facts observed. Under a pressure of 760 millims. the dry gases do not unite, while the wet mixture is explosive. The addition of a small quantity of nitrogen, carbonic acid, cyanogen, nitrous oxide, or carbon bisulphide, does not render the dry mixture inflammable.

The part played by the steam in the ordinary explosion of carbonic oxide and oxygen is similar to that of the nitric oxide in the sulphuric acid chamber. By suffering a succession of alternate reductions and oxidations the steam converts the carbonic oxide into carbonic acid. With very little steam this conversion is comparatively slow, so that in a nearly dry mixture there is no explosion, but the disk of flame is seen to travel slowly down the tube. With increasing quantities of aqueous vapour the rapidity of inflammation increases. An attempt* was made to measure this increase in the velocity of explosion by observing the pressures produced in the eudiometer when equal masses of carbonic oxide and oxygen were fired under nearly identical conditions of temperature, pressure, and cooling surface, but with different proportions of steam. These experiments showed that the velocity of explosion increased with increasing quantities of steam, but they gave no absolute value for the rates of explosion. To obtain absolute rates direct measurements were made, with a chronograph, of the time which elapsed between the passage of the spark through the mixture, and the breaking of a thin silver bridge at the other end of the explosion tube about 1 metre from the firing point. The explosion tube of 13 millims. diameter was soldered into a metal trough, so that each end projected a short distance from the end of the trough. The trough was filled with water at the desired temperature. To determine the rate of explosion of the nearly dry gases, the mixture was forced slowly into the explosion tube (1) through two sulphuric acid drying tubes, and (2) through two long tubes containing anhydrous phosphoric acid. By removing the phosphoric acid tubes the gases were tested in a less dry state. By making the gases bubble slowly through a wash-bottle containing water at different temperatures below the temperature of the explosion tube, the quantity of steam added to the mixture could be approximately measured.

The following table gives the results of these velocity experiments:—

* "British Assoc. Report," 1882.

Mean Rate of Explosion for the First Metre of Carbonic Oxide and Oxygen with different Quantities of Aqueous Vapour under Atmospheric Pressure.

Exploded at 10° C.		Exploded at 35° C.		Exploded at 60° C.	
Hygrometric state.	Rate in metres per sec.	Hygrometric state.	Rate in metres per sec.	Hygrometric state.	Rate in metres per sec.
Dried by passing slowly over fresh P_2O_5.	36	Dried by passing slowly over fresh P_2O_5.	44	Dried by passing slowly over fresh P_2O_5.	53
		Dried by passing over P_2O_5 used above.	69		
Dried by bubbling through two bottles H_2SO_4.	119	Dried by bubbling through 2 bottles H_2SO_4. Ditto	102 103	Dried by bubbling through 2 bottles H_2SO_4.	120
		Saturated at 6°. Ditto.	129 123		
		Saturated at 8°	155	Saturated at 8° Ditto	158 166
Saturated at 10°	175 176				
		Saturated at 12°	200	Saturated at 12°	211
		Saturated at 25° Ditto	225 226		
				Saturated at 35°	244
				Saturated at 50°	289
				Saturated at 60°	317

The velocity of explosion was found to increase rapidly from the point of inflammation. When a mixture of carbonic oxide and oxygen saturated with steam at 10° was exploded in a tube 13 millims. diameter, the velocity was found to be constant after the explosion had travelled 700 millims. along the tube. The constant velocity of the explosive wave attained under these conditions is rather over 1,500 metres per second. Berthelot and Vieille* give

* "Compt. Rend.," xcv, 151.

1,090 for the velocity of explosion of "dry" carbonic oxide and oxygen.

A comparison of the results obtained when dry carbonic oxide and electrolytic gas were exploded in a dried eudiometer with those obtained by Horstmann, and with those obtained by Bunsen,* using a chain of sparks to fire the mixture, revealed the fact that both changes in the shape of the vessel and changes in the initial pressure under which the gases are fired, affect the division of the oxygen. By continually increasing the initial pressure, a pressure is reached where no further increase affects the division of the oxygen. At and above this "critical pressure," the division of the oxygen is also independent of the shape of the vessel. The larger the quantity of oxygen used the lower the critical pressure is found to be.

When dry mixtures of carbonic oxide and hydrogen in varying proportions are exploded above the "critical pressure" with oxygen insufficient for complete combustion, an equilibrium is established between two opposite chemical changes represented by the equations—

$$(1)\ CO + H_2O = CO_2 + H_2$$
$$(2)\ CO_2 + H_2 = CO + H_2O.$$

So that at the end of the reaction the product of the carbonic oxide and steam molecules is equal to the product of the carbonic acid and hydrogen molecules multiplied by a "coefficient of affinity." This result agrees with Horstmann's conclusion. But Horstmann considers the coefficient to vary with the relative mass of oxygen taken.

A small difference in the initial temperature at which the gases are fired makes a considerable difference in the products of the reaction. This difference is due to the condensation of steam on the sides of the vessel during the explosion, and its consequent removal from the sphere of action during the chemical change. When the gases are exploded at a temperature sufficiently high to prevent any condensation of steam during the progress of the reaction, the "coefficient of affinity" is found to be constant whatever the quantity of oxygen used, provided that the hydrogen is more than double the oxygen.

The presence of an inert gas, such as nitrogen, by diminishing the intensity of the reaction, favours the formation of carbonic acid in preference to steam. When the hydrogen is less than double the oxygen the excess of oxygen cannot react with any of the three other gases present—carbonic oxide, carbonic acid, and steam—but has to wait until an equal volume of steam is reduced to hydrogen by the

* "Gasom. Meth.," 2te Auflage.

carbonic oxide. The excess of inert oxygen has the same effect as the inert nitrogen in favouring the formation of carbonic acid.

The variations in the coefficient of affinity found by Horstmann with different quantities of oxygen are due partly to this cause, but chiefly to the varying amounts of steam condensed by the cold eudiometer during the reaction in different experiments.

As the general result of these experiments it has been shown that when a mixture of carbonic oxide and hydrogen is exploded with insufficient oxygen for complete combustion, at a temperature at which no condensation of steam can take place during the reaction, and at a pressure greater than the critical pressure, an equilibrium between two opposite changes is established, which is independent of the quantity of oxygen taken, so long as this quantity is less than half the hydrogen. Within the limits marked out above, the law of mass is completely verified for the gaseous system composed of carbonic oxide, carbonic acid, hydrogen, and steam at a high temperature.

II. "On the Comparative Morphology of the Leaf in the Vascular Cryptogams and Gymnosperms." By F. O. BOWER, M.A., F.L.S. Communicated by W. T. THISELTON DYER, M.A., F.L.S. Received May 13, 1884.

(Abstract.)

Eichler, in his dissertation on the development of the leaf, defined the primordial leaf as the young leaf before internal differentiation or external distinction of parts; and further pointed out that subsequently two parts of it may be distinguished—the foliar base (blattgrund) which gives rise to the sheath and the stipules, if present, and the upper leaf (oberblatt) which developes into the simple branched lamina. The petiole is also, according to Eichler, derived from the upper leaf, though Goebel describes it as being intercalated between the two parts. The first part of the present paper is devoted to a discussion of this mode of treatment of the leaf. In accordance with the views clearly expressed by Sachs and others, the terms stem and leaf are to be regarded only as expressions denoting certain relationships of the parts of the *shoot;* the leaf is essentially an outgrowth from the stem. If this proposition be accepted, the same mode of morphological treatment ought to be applied to both. Now, in the treatment of the shoot as a whole, priority of importance is always attached to the mode of origin, and sequence of appearance of the several parts, while subsequent changes of conformation and dis-

turbance of their arrangement, resulting from peculiarities of the distribution and localisation of growth, are regarded as of but secondary importance. In treating of the leaf, however, this principle is not kept in view. The very distinction of foliar base and upper leaf is chiefly based upon results of intercalary growth which would be regarded as but of secondary importance in treating of the shoot at large. One of the objects of the investigations detailed in this paper was to ascertain, by comparative study of the lower forms, whether there is sufficient ground for this inconsistency of treatment. It is further pointed out that, though this treatment is not open to obvious objection in the case of simple leaves, in branched leaves the parts thus distinguished are not morphologically co-ordinate. On dividing the leaf into foliar base and upper leaf, a distinction is drawn between the lower part of the axis (so to speak) of the leaf and the whole of its upper branch system—a distinction which might be compared with that of the bole of a forest tree below the lowest branches from the whole of the upper part of the trunk with its branches of all orders; such a distinction would not lead to a true knowledge of the morphology of the tree, or of the relation of its parts one to another. It is found that a comparative study of the leaf of the lower vascular plants does not justify the continuance of this inconsistency in the method of treatment of axis and leaf, but rather that the leaves of the lower forms lend themselves to a consistent treatment throughout their length as branch systems. As we rise in the scale, the main axis of the branch system becomes gradually more differentiated as a supporting organ, distinct from the members of higher order which it bears, while the assimilating function chiefly devolves upon its flattened branches.

This being the case, this axis must be recognised by a distinct term: the name *phyllopodium* is proposed to designate *the main axis of the leaf exclusive of its branches* (pinnæ); thus the relation of the pinna to the phyllopodium is similar to that of the leaf to the axis. Three parts of the phyllopodium may be distinguished in complicated leaves, but this distinction is only to be drawn where a difference really exists; the basal portion may be called the *hypopodium*, and coincides with Eichler's "blattgrund;" the *mesopodium* is the equivalent of the petiole; the third part, or *epipodium*, differs from the "oberblatt" of Eichler in including only the upper part of the phyllopodium, *exclusive of its branches*. This method of treatment of the leaf is consistent with the treatment of the stem, while the parts severally distinguished are morphologically co-ordinate. As above stated, this method is amply borne out, and shown to be a natural one, by the study of the development of leaves in the lower vascular plants.

The second part of the paper is devoted to a detailed comparison

of the development of the leaf in a series of types of Vascular Cryptogams and Gymnosperms, beginning with those lowest in the scale; the third part points out the conclusions to be drawn from that comparative study. The chief facts and conclusions are as follows:—

In the simplest forms, the *Hymenophyllaceæ*, the apex of the young leaf, is, according to Prantl, flattened, and has the two-sided apical cell so characteristic of flattened organs; it branches chiefly, if not exclusively, by dichotomy. By the stronger development of one limb of each dichotomy a sympodial arrangement is produced, which appears in the mature leaf as an apparently well-defined pseudo-axis or phyllopodium; but since the branching is dichotomous, the phyllopodium is not clearly differentiated in the first instance from the less strongly developed limbs. It is characteristically a flattened structure, and is in most cases winged to its extreme base; and though the leaf is complicated, it has no peculiarity of conformation at the base to which the term hypopodium could with reason be applied.

In the majority of *Leptosporangiate Ferns*, exclusive of the *Hymenophyllaceæ* and *Osmundaceæ*, the apex of the phyllopodium still retains the two-sided apical cell so characteristic of flattened organs, but the branching of the leaf is at first monopodial, though there is sometimes a return to the dichotomous branching in the higher ramifications; its apical growth is in some cases unlimited. Thus the phyllopodium is here more clearly differentiated from the pinnæ, the structure of the apex is, however, still that characteristic of flattened organs; it is a winged structure to its extreme base, though the wings are in many cases reduced to mere lateral ridges.

In the *Osmundaceæ* the two-sided apical cell of the above-named ferns is replaced, during the earlier stages of development, by a *three-sided, conical, apical, cell;* and it is believed that in this respect they are unique among vascular plants. The young phyllopodium is thus typically a solid structure; it is still winged, and its branching is monopodial; it shows peculiar modifications of form at the base, but these are referable without difficulty to the fundamental winged structure; the apical growth is long continued, and in its upper parts the phyllopodium becomes flattened as in other ferns.

In *Angiopteris*, as an example of the *Marattiaceæ*, there is no single apical cell occupying the bulky apex of the phyllopodium, but in its place there is a group of four cells, and in this respect it approaches the higher types of vascular plants. The phyllopodium is thus a solid structure from the first, its apical growth is limited, and its branching monopodial, though the order of succession of the pinnæ is here, as in other ferns, strictly *acropetal.* The peculiar stipular structure at the base is referable in the light of similar structures in *Todea* to a modification of the winged structure.

Finally, in the *Cycadaceæ* the rounded apex of the phyllopodium is covered by a definite layer of dermatogen; its growth is never very distinctly apical, though in *Cycas*, and perhaps in *Dioon*, it is more so than in other genera; it is winged throughout. Its branching is in all cases *Monopodial*, but the order of succession is, with exception of the upper pinnæ in the above-named genera, *basipetal*.

From the above and other facts, it is clear that in the series of plants above dealt with, a progressive differentiation may be traced of the phyllopodium on the one hand as a supporting organ, and on the other hand of the other members of higher order, which develope as flattened organs. The phyllopodium in the first named families is typically a flattened organ with unlimited apical growth; in the higher members of the series it is a solid structure from the first, and its apical growth is arrested at an early period. Moreover, while in the lower forms it branches dichotomously, in the higher members of the series it branches monopodially, and is thus clearly distinguished at all times from the members of higher order which it bears. It is thought that this progressive differentiation of the phyllopodium as a supporting organ among the similar members of a branch system may throw light upon the mode of origin of the axis as a structure bearing leaves; and it is suggested that this analogy is at least closer than any that can be drawn from the study of the leafy *Muscineæ*. As the phyllopodium gradually asserts, and in the higher forms of the above series, maintains its identity among the branches of the leaf, so the axis may have differentiated itself as a supporting organ from among members similar to itself in origin and development.

On ascending through this series of plants, there is further seen a gradual restriction of the apical growth, which in the simpler ferns is sometimes unlimited. This leads on towards the higher vascular plants, in which the apical growth of the leaf usually ceases at an early stage, the general conformation of the leaf being due, as above pointed out, in much greater degree to intercalary growth. Since this change is gradual, no important difference of morphological treatment of the leaf in the lower and the higher forms ought to be based upon it. Now the leaves in the lower forms naturally lend themselves to a treatment throughout as branch systems, while there is even in some complicated fern leaves no structure which can warrant a distinction of the foliar base from the upper leaf. On these grounds, and in the light of this comparative study, it is concluded that the recognition of the phyllopodium, and treatment of the whole leaf as a simple branch or as a branch system, is in accordance with the true nature of the leaf as seen in all vascular plants: while at the same time, if this method were adopted, the whole shoot would then be subjected to consistent morphological treatment, since the relation of the pinnæ to the phyllopodium is similar to that of the whole leaf

to the axis which bears it. This may be shown simply in tabular form thus—

- Shoot.
  - Axis.
  - Leaf.
    - Phyllopodium.
    - Pinna.

III. "On the Changes and ultimate Fate of the Blastopore in the Newt (*Triton cristatus*)." By ALICE JOHNSON, Newnham College, Cambridge. Communicated by Professor MICHAEL FOSTER, Sec. R.S. Received May 15, 1884.

In a paper lately published on "The Origin of Metameric Segmentation," Mr. Sedgwick mentions that he was led to conclude from surface views of a large number of stages that the blastopore of the newt (*Triton cristatus*) does not close, but persists as the anus. He suggested to me afterwards that I should attempt to test this observation by cutting sections of the embryos, and my results fully confirm what he previously stated.

Scott and Osborn, in their account of the development of the newt, describe a posterior dilatation of the medullary canal, the sinus rhomboidalis, which remains open for some time after the rest of the canal has closed. They say that its folds enclose the blastopore, and that, therefore, when they come together, a neurenteric canal is formed. Their account of the exact date of the closure of the medullary folds in this region is a little obscure, but seems to indicate that the event takes place while the number of protovertebræ is quite small, and before the rudiments of the visceral arches and tail have appeared.

Hertwig figures an open blastopore at a slightly later stage than this, but, judging from his surface views of the embryo, it is situated at the hind end of the tail.

At a stage when the medullary folds are widely apart, the slightly elongated blastopore is placed at the hind end of the body. While the folds are approaching one another the dorsal surface grows faster than the ventral, so that the blastopore is carried round on to the ventral surface. When the folds have completely coalesced, and before there is any trace of a tail, the blastopore is placed at some little distance from the hind end, which is much swollen and spherical in outline.

In transverse sections of the stage just described we find the blastopore actually communicating with a cavity in the midst of the yolk-cells. This cavity is so exceedingly narrow that it can hardly be

traced into connexion with the middle part of the gut. (In longitudinal sections it appears as a mere line, but its continuity with the rest of the gut can then be clearly followed.)

For some distance behind the blastopore all the layers are fused in the middle ventral line. We find, in fact, a *primitive streak*, exactly comparable with that of the amniotic vertebrata. Passing further backwards, a large solid roundish mass is gradually marked off in the middle line from the lateral plates of mesoblast and the hypoblast yolk-cells. This mass gradually acquires a lumen, and is finally continued round into the medullary canal on the dorsal surface. Thus there is no neurenteric canal at this time, and I have equally failed to find it at any other stage.

The tail next buds out from the region *behind* the blastopore. It is a small roundish knob, its apex pointing downwards and forwards on account of the ventral curvature of the whole body. Scott and Osborn describe it as "an unsegmented mass of mesoblast." I should prefer to call it an outgrowth of the front end of the primitive streak, in which the same features that I have described in the previous stage are now seen still more clearly. As it passes back from the dorsal surface round the hind end of the body, the medullary canal loses its lumen and becomes a solid cord of epiblast, distinct from the notochord and lateral plates of mesoblast. A few sections further on, it becomes indistinguishably fused with both these structures, and a mass of cells is thus formed, from which a knob representing the rudiment of the notochord projects into the central yolk-cells. Passing still in the same direction along the ventral surface towards the blastopore, the notochordal rudiment disappears and the fused mass of cells becomes shorter from above downwards, and also compressed from side to side. The cavity of the hind gut is almost obliterated in the hind part of the ventral side of the body, and then widens out again in front. Here it is a slit, having its dorsal wall in close contact with its ventral wall, but comparatively broad from side to side. Further forwards it becomes wider from above downwards and narrower from side to side, till it passes at the root of the tail into the blastopore. The blastopore marks the extreme front end of the primitive streak on the ventral surface.

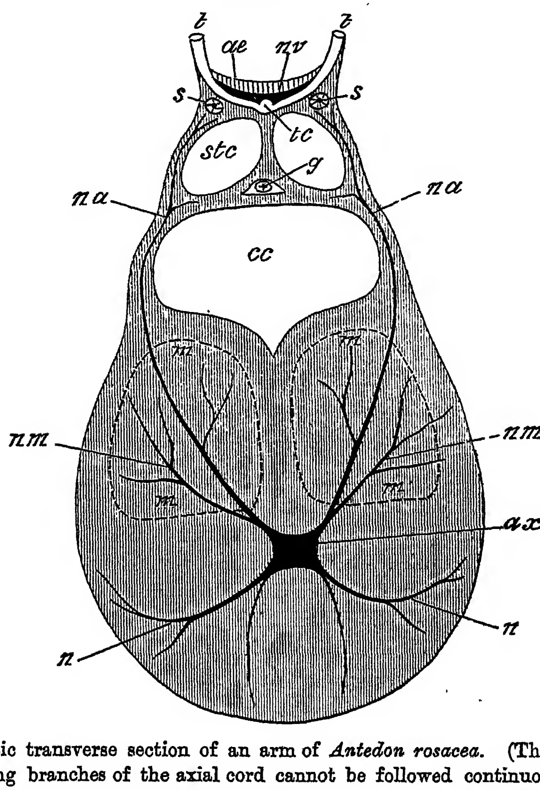
The solid condition of the hinder part of the medullary canal for a short region before it becomes indistinguishably fused with the primitive streak is described by Gasser in the bird, and he asserts that the lumen is gradually continued back as the medullary canal is differentiated out of the primitive streak. The description would, I think, apply equally well to the newt.

I have not yet made out satisfactorily the condition of the primitive streak in the early stages, but I hope to be able to elucidate this later, and to publish figures illustrating my results.

IV. On the Nervous System of the Crinoidea." By WILLIAM B. CARPENTER, C.B., M.D., LL.D., F.R.S. Received May 20, 1884.

In a Memoir "On the Structure, Physiology, and Development of *Antedon* (*Comatula*, Lamk.) *rosacea*," presented to the Royal Society in 1865, I stated* that I had ascertained that the cord (fig. 1, *g*) lying

FIG. 1.



Diagrammatic transverse section of an arm of *Antedon rosacea.* (The ventral or ascending branches of the axial cord cannot be followed continuously in any single section.) *ax*, axial cord, giving off pairs of branches, *n*, *n*, which proceed towards the dorsal aspect of the arm; *nm*, branches distributed on the ends of the muscular bundles, whose position is marked out by the dotted lines *m*, *m*; *na*, ventral branches; *cc*, cœliac canal; *stc*, subtentacular canal; *g*, genital rachis; *tc*, tentacular canal or water-vessel, giving off branches to the tentacula, *t*, *t*, between which lies the ambulacral groove, whose floor is covered by a thick ciliated epithelial layer, *ae*, immediately beneath which is the riband-like band, *nv*, supposed to be the ventral nerve; *s*, *s*, sacculi.

* "Philosophical Transactions," 1865, p. 705.

between the two principal canals (*stc* and *cc*) in the arms of Crinoidea, which had been regarded by Professor J. Müller as a nerve, really belongs to the reproductive apparatus; and further, that I had been led to regard as a nerve-trunk the solid cord (*ax*) which traverses the axial canal of each calcareous segment of the rays and arms, through finding that this cord gives off a regular system of branching fibres (*nm*, *nm*) to the muscular bundles (*m*, *m*) which intervene between the calcareous segments, and which flex the arms by their contraction.

In a further communication on this subject made to the Royal Society at the beginning of 1876, I supported this view by experimental evidence;* showing that in an eviscerated specimen of Antedon, irritation of the quinquelocular organ (contained in the centro-dorsal basin) from the walls of which the radial cords proceed, produces a sudden and simultaneous contraction of the flexor muscles of the arms, similar to that which I had mentioned in my Memoir (§ 13) as resulting in the natural condition of the animal from irritation of its oral pinnules.

That the supposed nerve of Müller is really what I stated it to be—a genital rachis—had been independently ascertained by Professor Semper, and is now universally acknowledged. But my other conclusion has not gained the same acceptance. Coincidently with the communication to which I have last referred, it was affirmed by Professor Greef of Marburg and by Dr. Ludwig, that the real nerve in the arms of Crinoïdea is (as in other Stellerida), a fibrillar band (fig. 1, *nv*) that lies beneath the epithelial floor (*ae*) of the ambulacral (ventral) furrow of the arms; a conclusion at which Professor Huxley had independently arrived. And this view is now very generally received and taught in Germany.

In a third communication which I shortly afterwards† made to the Royal Society, I referred to this doctrine; and, whilst admitting the probability that this sub-ambulacral band is really a nerve, I adduced what seemed to me conclusive proof that it cannot be the nerve through which the motor apparatus of the arms is called into action. For, in the first place, it is far removed from this apparatus in position; being separated from the muscles by the triple canal-system, and not being connected with them (so far as can be discerned) by any branching fibres. And, further, the loss of the visceral mass (which contains the central ring of this ambulacral nerve-system) was not found in the least degree to interfere with the rhythmical swimming actions of the animal; whilst a division of the ambulacral nerve in any individual arm produced no paralysis of that arm.

On the other hand, I stated that my son (who was then working in the laboratory of Professor Semper at Würzburg) had fully confirmed

* "Proceedings," Jan. 20, 1876, p. 226.

† *Ibid.*, April 6, 1876.

—by means of thin sections—the statement I had made ten years previously, as to the regular transmission of pairs of branches (fig. 1, *nm, nm*) from the axial cord of the arms to their successive pairs of flexor muscles; and I adduced what seemed to me conclusive experimental proof that these cords, which radiate from the wall of the central quinquelocular organ, and are further connected with each other by a commissural ring, have a motor function.

In the first place, I argued that the extraordinary co-ordination which is manifested in the active swimming action of Antedon, when it spontaneously leaves, or is detached from, the anchorage afforded by the grasp of its dorsal cirri, cannot be accounted for without a definite direction from a nervous centre. That this centre is not in a circum-oral ring, is clear from the continuance of the regular movements after the complete evisceration of the animal. On the other hand, that it is contained within the centro-dorsal basin, was indicated alike by the coiling-up of the arms when the quinquelocular organ was irritated, and by the complete paralysis of the flexor muscles which followed the removal of the centro-dorsal basin with its contents. And, further, the destruction of a portion of the axial cord of an arm, the ventral nerve being left uninjured, was shown to be followed by complete paralysis of the muscles of that arm beyond the injured part.

The anatomical and the experimental evidence that the quinquelocular organ, with its radiating and branching cords, constitute the motor nervous system of the arms, being thus in complete harmony, I ventured (p. 454) to profess myself "at a loss to understand what is the superior probative force of the evidence which is universally held to justify the assignment of such functions to the brain and spinal cord, and the white solid cords proceeding from these centres, in a Vertebrate animal."

That the sub-ambulacral band of Ludwig is also a nerve (as homology would indicate), I thought not improbable; but looking to its immediate proximity to the sensory (ventral) surface, and to the absence of any connexion with the muscular apparatus, I thought that it might probably be an afferent nerve, "the functions of the single trunk of the Asterida being here divided between two, an afferent and a motor, just as, in Man, the double function of an ordinary spinal nerve is divided in the head between the fifth and seventh pairs."

During the eight years which have elapsed since these statements were made public, it might have been expected that my conclusions would have been either accepted or controverted. But the question has been considered by many eminent Zoologists, especially in Germany, as one which is so conclusively settled by Homology, as not

to be a matter for discussion; it being impossible (in their judgment) that the axial cords of the arms should be nerves, whatever may be the anatomical and experimental evidence that they are. I would submit, however, that the possibilities of Nature are not limited by the dicta of her interpreters;* that anatomical and experimental facts are not to be set aside by preconceived theoretical opinions; and that the morphology of the Crinoidea has to be settled upon the basis of their own organisation, before it is brought into comparison with that of other Echinodermata. Now the question whether the axial cords of Crinoidea do, or do not, form part of their nervous system, has to be decided: *first*, by their Histological character; *secondly*, by their Anatomical distribution; and, *thirdly*, by Physiological evidence; and on each of these points I have now a large body of new evidence to adduce, derived from the careful and minute investigations on which my son, Dr. P. Herbert Carpenter, has been continuously engaged during the last eight years. Of the results of these investigations, which are scattered through the various papers he has published on "Crinoid Morphology," I shall now present a summary, arranged under the above heads; referring to those papers† for a more detailed statement of them.

Histological Character.—Although the axial cords do not consist of tubular nerve-fibres—their substance being essentially protoplasmic, and showing but an indistinct fibrillation when hardened in spirit—yet, scattered through these cords and their branches, Dr. P. H. Carpenter has found distinct bi-polar and multi-polar cells; and he has further ascertained that the sub-ambulacral band presents a histological character so precisely identical, notwithstanding the difference of its origin, as to afford a strong presumption that if the latter is a nerve, the former likewise is so. On the other hand, the axial cords, which are regarded by Ludwig as merely unconsolidated portions of the basis-substance of the calcareous segments, differ essentially from that substance histologically.

Anatomical Distribution.—Nothing can be more marked or more constant than the distribution of the branches (fig. 1, *nm*, *nm*) of the axial cords to the very definite inter-segmental muscular bundles of the arms and pinnules, alike in the free and in the pedunculate

* Every one familiar with the History of Science knows how often such *à priori* assumptions have been made and disproved.

† "Remarks on the Anatomy of the Arms of the Crinoids," in "Journal of Anatomy and Physiology" (1876), vol. x, p. 584, and vol. xi, pp. 87–93; "On the Genus *Actinometra*," in "Transactions of Linnæan Society," Second Series, Zool., vol. ii, pp. 32–37; "On the *Comatulæ* of the 'Challenger' Expedition," "Proc. Roy. Soc.," March 6, 1879, pp. 394–395; "The Minute Anatomy of the Brachiate Echinoderms," in "Quarterly Journal of Microscopical Science," vol. xxi, pp. 188–193, and vol. xxiii, pp. 614–616.

Crinoids. Moreover, in the dorsal cirri of Comatulidæ, which have a markedly prehensile power, but have no definite muscular bundles, the axial cords send branches into the contractile substance that serves the purpose of muscles. Branches of the axial cords (*na, na*) also proceed to the lateral surfaces of the soft parts that lie on the ventral side of the arms and pinnules, and are traceable to the very lips of their ambulacral grooves, forming also an extensive plexus along the sides of the ambulacral grooves of the disk. On the other hand, the sub-ambulacral nerves, which in Ophiurida send very distinct branches to the muscles of the arms, send no such branches to the arm-muscles of Crinoidea. And thus the evidence furnished by anatomical distribution as to the source of the nerve-power which calls those muscles into contraction, is alike positive in regard to the axial cords, and negative in regard to the sub-ambulacral nerves. At the same time, the distribution of the branches of the axial cords to the perisome of the soft parts of the arms and pinnules, would indicate that these have an afferent or sensory function.*

Physiological Evidence.—The inquiries of Dr. P. H. Carpenter, having been entirely limited to the anatomical examination of spirit specimens of Crinoidea, do not afford any direct confirmation of the statements I formerly made as to the actions of living Antedons; but they furnish most remarkable confirmatory evidence of an indirect kind—that, namely, which may be deduced from what Cuvier termed "Experiments prepared for us by Nature." For having met with numerous cases in which the ambulacral groove and the tentacular apparatus are wanting, whilst the arms and pinnules showing this deficiency are normally constructed in other respects, he has invariably found that the ventral or sub-ambulacral nerve is alike deficient, while the axial cord and its branches have their usual distribution. Among these cases, the following may be specified:—

a. The long pinnules which come off from the second brachial segments of *Antedon rosacea*, and which (from the manner in which they arch over the mouth during life) have been distinguished as oral pinnules, are destitute of the tentacular apparatus—as I pointed out in my original Memoir (§ 16). This peculiarity has been found by my son to be a general character of the genus Antedon; and he has further shown that, with the deficiency of tentacles, there is also an absence of the ordinary ciliated epithelium of the ambulacral groove, and of the subjacent nerve and nerve-vessel.†

* The observations of Dr. P. H. Carpenter upon the distribution of the branches of the axial cords, have been fully confirmed by those of M. Edmond Perrier; who has been led by his own independent investigations on *Antedon rosacea* to the full acceptance of the nervous character of these cords, notwithstanding his opposite prepossession. (See "Comptes Rendus," July, 1883, tome xcvii, p. 187.)

† "Journal of Anatomy and Physiology," vol. xi, October, 1876, p. 89.

b. It has been noticed, as well by Ludwig as by Dr. P. H. Carpenter, that in the terminal segments alike of the arms and of the pinnules of *Antedon Eschrictii*, there is a similar want of the tentacular apparatus, with an obliteration of the ambulacral grooves by the approximation and fusion of the elevated folds of perisome at their sides; and that here also the sub-ambulacral nerve is absent.

c. The most remarkable case of this kind, however, is presented by that aberrant type of Comatulidæ which is distinguished generically as *Actinometra*. This genus differs from Antedon in the excentricity of its mouth: the anal orifice being generally in or near the centre of its ventral disk, whilst the mouth lies near its margin. This curious disposition is not related to any departure from radial symmetry in the structure either of the calcareous skeleton, of its muscular apparatus, or of the axial cords whose branches are distributed to the muscles and perisomatic surface of the arms; but it is associated with a very marked irregularity in the disposition of the tentacular and ambulacral apparatus. For, whilst in the arms given off from the *oral* side of the disk, some pairs of pinnules are usually destitute both of tentacles and of ciliated ambulacral grooves, this deficiency is generally complete in a large proportion, not only of the pinnules, but of the arms arising from the *aboral* side of the disk, sometimes amounting to one-half of the entire circle, and occasionally also on the disk itself. And wherever the tentacles and ambulacral groove are wanting, the sub-ambulacral nerve also is absent. Yet we are assured by Professor Semper, who kept *Actinometræ* for weeks together in his aquaria, that "he never saw the least trace of any irregularity in the alternating movement of their arms while swimming;" the non-tentaculated members, notwithstanding the want of sub-ambulacral nerves, acting precisely like the tentaculated.*

Thus, then, the "Experiment prepared for us by Nature," in the subtraction of the sub-ambulacral nerve-system from certain pinnules and arms of Comatulidæ, entirely confirms the conclusions which I drew from my artificial induction of the same physiological condition. For it is, of course, impossible that their oral nerve-ring can minister to the general sensori-motor actions of arms and pinnules which receive no radial extensions of it; and yet the character of those actions (as I have already pointed out) so distinctly indicates their dependence on the originating and co-ordinating power of a nerve-centre, and on the internuncial power of nerve-cords, that we must seek elsewhere for a nervous mechanism on which they depend. Those who deny that this can be furnished by the axial cords, by the dorsal centre from which they radiate (with the remarkable annular commissure on its primary trunks), and by their minute ramifications

* See Dr. P. Herbert Carpenter's Memoir on the genus *Actinometra*, in "Linnæan Transactions," New Series, vol. ii, Zoology, p. 36.

in the muscular bundles and perisomatic surface, have to account for these two facts in the ordinary life-history of uninjured animals:—*first*, the immediate and consentaneous contraction of the hundreds (or thousands) of arm-muscles in Antedon, called forth by irritation of the oral pinnules; and, *secondly*, the performance of swimming movements as regular as those of Antedon, by the non-tentaculated arms of Actinometræ—notwithstanding the absence, in both cases, of what is affirmed to be their sole nervous supply.

Thus, while the doctrine that the remarkable sensori-motor endowments of the Crinoidea depend upon their *ventral* nervous system, consisting (as in Asterida and Ophiurida) of an oral ring with radial branches, is supported only by a theoretical homology, it is in direct contradiction to the following facts:—

1. The absence of any branches from the sub-ambulacral nerves to the muscular apparatus of Crinoidea generally.

2. The absence of sub-ambulacral nerves from those pinnules of Antedon which are most distinguished by their sensory endowments.

3. The absence of sub-ambulacral nerves from a large proportion of the arms of Actinometræ, which, nevertheless, take their full share in the co-ordinated swimming movements of those animals.

4. The continued performance of these movements by Antedons from which the whole visceral mass, including the oral ring, has been removed, and by arms whose sub-ambulacral nerves have been cut near their base.

On the other hand, the dependence of the general sensori-motor endowments of Crinoidea upon what I have described as their *dorsal* nerve-system, is a doctrine which has been found to harmonise alike with every fact that the most careful and minute study of their organisation has brought to light, with the results of the "Experiments prepared for us by Nature" in the varieties of that organisation, and with those of such experiments upon the living animals as would be deemed conclusive in other cases. It is opposed only by a theoretical homology, a preconceived notion of what Crinoids ought to be,* which was adopted (as Dr. P. H. Carpenter has perti-

* Thus Baudelot, who was searching for the nervous system of the Crinoidea, and traced out the whole system of dorsal cords with their pentagonal commissure (apparently in ignorance of what I had previously done), while remarking that "dans leur disposition aussi bien que dans leur structure ces parties offrent une analogie presque complète avec les cordons nerveux des autres Échinodermes," nevertheless affirms that "*évidemment elles n'appartiennent point au système nerveux.*" ('Archiv. de Zool. Exper. et Gén.," tome i, p. 211.)

[Since the above was written, Dr. P. H. Carpenter has drawn my attention to a recent paper by Dr. Weinberg on the Morphology of living Crinoids ("Der Naturhistoriker," Mar.—Jun., 1883, pp. 266—307), in which Dr. P. H. Carpenter's descriptions (with illustrative figures) of the muscular branches of the radial cords are treated as "suppositions;" while his account of the absence of tentacles, of the

nently remarked) without any sufficient knowledge of the anatomy of this most interesting group. Further, that this *dorsal* nerve-system is the fundamental and essential sensori-motor apparatus of Crinoidea, and that their *ventral* nerve-system is secondary and accessory, is indicated by the universal presence of the former in every arm and pinnule, while the latter is frequently absent. And that the function of this latter is limited to the control of the tentacular apparatus, appears probable from the constancy of its association with that apparatus; being invariably present in those arms and pinnules which are provided with tentacles, and absent in those which are destitute of them.

In conclusion, I would remark that the question whether these axial cords do or do not constitute parts of the fundamental nervous system of Crinoidea, is one of far-reaching interest; since it obviously affects our whole conception of the morphology of the group. If I am right in my contention, the centre of the nervous system of the Crinoidea has its seat in that Stem which is the most distinctive feature of their structure. For the quinquelocular organ that lies in Antedon within the centro-dorsal basin, is only an expansion of the soft axis which occupies the central canal that extends through the entire length of the stem; repeating on a larger scale a similar dilatation that occurs at every node from which a circlet of cirri is given off. And the radial skeleton of the stem, of the calyx, of the arms, and of the pinnules of a Crinoid, is even more completely built up on this elongated nerve-centre and its radial extensions, than is the longitudinally segmental skeleton of a Vertebrate animal upon its cranio-spinal axis,—a consideration which must be constantly kept in view in any attempt to trace out the homologies of Crinoidea with other Echinodermata. To myself it has always appeared that

ambulacral epithelium, and of the ventral nerve, in a large proportion of the arms of *Actinometra*, is altogether ignored. As Dr. Weinberg seems to justify his disbelief of Dr. P. H. Carpenter's description, by his own failure, and that of Dr. Ludwig, to verify them on *Antedon rosacea*, it may be well for me to state that its accuracy has been verified by careful examination of Dr. P. H. Carpenter's preparations, not only by myself, but by many other experienced Microscopists in this country; whilst, as already mentioned (p. 71, *note*), Professor Perrier has been led by his own independent investigations to accept my own and my son's statements as fully borne out by microscopical evidence. As Dr. Weinberg, though he has had the opportunity of studying *Antedon rosacea* alive, and of thereby refuting my experimental results, if erroneous, refrains from discussing them, and as he also ignores the fact (though vouched for by Ludwig) that the ventral nerve is wanting in the peculiarly sensitive oral pinnules of *Antedon*, it seems as if his confidence in his theoretical Morphology blinds him to every fact which conflicts with this. The complete confirmation of my experiments by Professor A. M. Marshall and Dr. Carl F. Jickeli (see ADDENDA) may perhaps render them worthy of his more serious consideration (June 28).]

they differ much more widely both from Asterida and Ophiurida, than those Orders differ from each other; and while all recent researches tend to show that Crinoidea are closely allied to Blastoidea and probably to Cystidea, they bring into view their points of difference from all Echinoderms the aspect of whose mouths is downward,—a distinction long since put forward by Leuckart as one of fundamental value.

It was pointed out nearly twenty years ago by Sir Wyville Thomson and myself, that the canalisation or non-canalisation of the calcareous segments of the Crinoidal skeleton, for the passage of the axial cords, affords a distinctive character by which its proper *radial* portion can be differentiated from the accessory pieces by which its arrangement is often complicated and obscured. And the practical value of this character has been recognised by various students of the extinct types of the group,—with this modification, that among the Paleocrinoids the axial cords often lie in grooves which have not closed-in to form canals, just as I have shown to be the case in Antedon at a certain stage of the development of the radials. It is obvious that the morphological value of this character becomes much greater, if the axial cords are nerve-trunks which call into action the complicated muscular apparatus of the arms, than if they are to be regarded (with Ludwig) as merely unconsolidated portions of the general basis-substance of the calcareous skeleton.

Another point of interest—Physiological rather than Morphological—is the existence of a definite nervous system, possessed of great functional activity, which yet shows very little histological differentiation. It can scarcely be doubted, I think, that there is here no definite distinction between ganglionic centres and nerve-trunks; almost every part of the apparatus being probably capable of originating as well as of conducting. The peripheral branches distributed to the perisome will, of course, be those by which sensory impressions will be received; while the branches distributed to the muscles will be those which call forth their motor activity. But that the axial cords of the arms are not *mere* conductors, seems proved by the performance of active spontaneous movements by arms which have been for several days detached from the body. And the connexion of these cords with each other in the annular commissure and in the quinquelocular centre, would seem to have reference rather to the co-ordination of actions which would be otherwise independent, than to a derivation of nerve-power from either of those sources.

I cannot but think that I have now given sufficient reason why the question I have raised should be no longer ignored, but should be reconsidered in the light of the new facts and arguments I have adduced in support of my views. Those who refuse to accept them, are bound, I think, either to disprove the facts, or to show that

my deductions from them are unsound. To assert that they are "evidently" erroneous, is clearly an unscientific mode of disposing of them, unworthy of any real lover of truth.

ADDENDA.

(June 11.)—I am permitted by Prof. A. M. Marshall, of Owens College, to state that having, during a recent visit to Naples, repeated for himself at Dr. Dohrn's Zoological Station the experiments which I performed there in 1876 upon the Nervous System of *Antedon rosacea*, he found their results confirmatory of my own in every particular; whilst he was further led to assign an *afferent* as well as a motor function to the *dorsal* nerve-system, as I had myself been led to do by the absence of the *ventral* nerve in the aboral arms of *Actinometræ*. Professor Marshall informs me that he hopes to publish an account of his experiments in the next number of the Quarterly Journal of Microscopical Science.

(June 28.)—I also learn from Prof. Marshall and my Son, that a further experimental confirmation of my conclusions has been recently published by Dr. Carl F. Jickeli of Jena.* Having investigated the subject four years ago at Trieste, he not only repeated and verified my experiments, but varied them by the use of electric stimulation. He found that when this was applied to the ambulacral groove of a detached arm, it produced no effect; but that if applied to the axial cord, it called the muscles of the arm and pinnules into contraction, so as to produce flexure, even though the arm had previously shown no signs of life. If applied to the axial cord of a cirrus, electrical stimulation threw the cirrus into tetanic contraction. Further, Dr. J. found that while the application of caustic to the ambulacral groove of an arm had no effect in preventing the excitation of flexure by electric stimulation of the axial cord, the application of caustic to the axial cord itself caused a straightening of the arm and a cessation of its movements, as if by the killing of its nerve.

These results, says Dr. Jickeli, can be explained in no other mode than Dr. Carpenter's; and he further states that his histological examination of the axial cord has satisfied him of its nervous character.

In claiming to be the first, after my Son, who has publicly adopted my view, Dr. J. seems unaware that Professor Perrier has been led to accept it, by his study of the anatomical distribution of the pairs of branches given off from the axial cord. (See note, p. 71.)

W. B. C.

The Society adjourned over the Whitsuntide Recess to Thursday, June 19.

* Über das Nervensystem und die Sinnesorgane der *Comatula Mediterranea*," in "Zool. Anzeiger," 7 Jahrgang, No. 170, p. 346.

June 12, 1884.

The Annual Meeting for the Election of Fellows was held this day.

THE PRESIDENT in the Chair.

The Statutes relating to the election of Fellows having been read, General Clerk and Sir Erasmus Ommaney were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

Allman, Prof. George Johnston, LL.D.
Balfour, Prof. Isaac Bayley, D.Sc.
Baxendell, Joseph, F.R.A.S.
Bell, James, F.I.C., Ph.D.
Hartley, Prof. Walter Noel, F.R.S.E.
Herschel, Prof. Alexander Stewart, M.A.
Hudleston, Wilfrid H., M.A.
Lamb, Prof. Horace, M.A.
McKendrick, Prof. John G., M.D.
Ransome, Arthur, M.D.
Roy, Prof. Charles Smart, M.D.
Rücker, Prof. Arthur William, M.A.
Thomson, Joseph John, B.A.
Warren, Sir Charles, Colonel, C.M.G.
Watson, Prof. Morrison, M.D.

Thanks were given to the Scrutators.

June 19, 1884.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Professor George Johnston Allman, Professor Isaac Bayley Balfour, Dr. James Bell, Professor John G. McKendrick, Dr. Arthur Ransome, Professor Arthur William Rücker, Mr. Joseph John Thomson, and Colonel Sir Charles Warren were admitted into the Society.

The President read the following letter which had been received in acknowledgment of the Memorial agreed to by the Society at their meeting on April 3rd :—

Whitehall,
30th April, 1884.

SIR,

I have had the honour to lay before the Queen the loyal and dutiful Address of the Royal Society of London for Improving Natural Knowledge, on the occasion of the death of His Royal Highness Prince Leopold, the Duke of Albany, K.G., and I have the satisfaction to inform you that Her Majesty was pleased to receive the Address very graciously.

(Signed) W. V. HARCOURT.

The President of the Royal Society.

THE BAKERIAN LECTURE—"Experiments on the Discharge of Electricity through Gases: Sketch of a Theory"—was delivered by Dr. SCHUSTER, F.R.S.

The following Papers were read :—

I. "On the Structure and Development of the Skull in the Mammalia. Part II: Edentata." By W. K. PARKER, F.R.S. Received May 26, 1884.

(Abstract.)

My former paper on the structure and development of the mammalian skull was published in the "Philosophical Transactions" in 1874; it was on that of the Pig.

Since then, whilst gathering fresh and fresh mammalian materials, the greater part of my actual work has been on the skull of the other classes.

There have been two reasons for this: first, the difficulty of obtaining subjects for dissection, and then the fact that the mammalian skull is the most difficult of interpretation, and asks for an interpreter who is quite familiar with that of more simple types.

Now, I hope to take up Order after Order of this, the highest group of the Vertebrata. Of course this will employ me for many years to come.

I would, if it were possible, take the lowest types first, namely, the Monotremes or "Prototheria," but this cannot be done. Even the Marsupials or "Metatheria" will have to come after the Insectivora, that is, they will be treated of in the next paper but one.

I am not of opinion that this irregular way of working has any harm in it; when once the facts have been gathered and arranged the mind will be able to draw its deductions; if the garners be but full, a little irregularity in the harvesting will not count for much.

The materials for the present paper have taken me more than twenty years to collect. Hearty thanks are due to those friends* who have helped me according to their power.

I have worked out the skull in several embryos of the Armadillos, these belong to two genera and three species.

In the Bradypodidæ (Sloths) I have had two stages of the embryo, and two of the young, these belong to two genera and three species.

In the Anteater (*Cycloturus*) I have worked out the skull in two young specimens; in the Pangolins (Manidæ) in an early and a latter embryo, in the new-born young, and in the adult; and in *Orycteropus* in the nearly ripe embryo.

I now offer, in the main paper, the results of this piece of research; this has not been done, however, until much more of the same kind of work has been either finished or got well in hand, as I am anxious for the structure of the skull in the various orders to have become familiar to me for the sake of comparison, expressed or unexpressed.

I have come to the conclusion that the Edentata are nearer of kin to the Monotremata than to the Marsupialia, and that if they did, as indeed they must have done, pass through a Metatherian, or Marsupial stage, they did not utilise it but ran through it in an *abbreviated pre-natal* stage.

Of course the remarkable modification of their jaws, due to abortion, and in some cases complete suppression, of their teeth,

* Professors Flower and Mivart, Drs. Günther and Sclater, and H. M. Ward, Esq., F.Z.S.

is that which makes these forms so abnormal to the morphologist, as well as to the zoologist.

As it happens, the most primitive form of Mammalia existing, the Prototheria (*Ornithorhynchus* and *Echidna*) are also abnormal on the same account, and thus the best standard, existing, by which to measure the height of the platform on which we find the Edendata, is not itself normal, or straight, or perfect.

Now none of the Metatheria or Marsupials have suffered from this kind of degenerative specialisation; they, therefore, come in well as standards of measurement and comparison for the Insectivora, next above them, but are of little use here among the Edentata.

Professor Flower, after working out the general anatomy of this group ("Proc. Zool. Soc.," 1882, pp. 358—367), has come to the conclusion that the Edentata of the Old World have little to do with those of the New.

That sounds like a hard saying to one not familiar with the structure of the group; it did so to me, no long time since, although what I had done at the group, long ago, went to prove the same thing; now, however, I am quite satisfied of the truth of my friend's deductions.

The Neotropical Edentata hold together much more than might have been expected. The Armadillos are the most isolated; but much as the Aard-Vark of the Cape looks like an archaic Armadillo without armour, he is not more than a very distant relative of the modern armed Armadillos.

Indeed, the curious coincidence that I have found between the structure of the Aard-Vark and that of a large Insectivore from a contiguous region, namely, the *Rhyncocyon* from Zanzibar, leads me to suspect that the Cape Anteater is an offshoot from the same stock, and is, indeed, the only Edentate that can be looked upon as probably arising originally from a Metatherian or Marsupial stock, like the Insectivora.

The other Palæotropical Edentata—the Pangolins—are perhaps still more isolated than the Aard-Vark, but they have not come so near extinction, and are found in more than one continent of the Old World.

If the term *Reptilian* might be applied to characters seen in any Placental Mammal, it might to what I find in this. This creature has most remarkable correspondences with the Reptilian group. Of course, the scaly covering is merely mimetic of the Lizard's scales, and is in reality made up of cemented hairs; that may pass; but not the structure of the sternum in some species, with its long "xiphisternal horns," as in the *Stellionidæ*, and the cartilaginous abdominal ribs, as in the Chameleons and some other kinds. (See my memoir on the "Shoulder-girdle and Sternum," Roy. Soc., 1868, Plate 22, fig. 13.)

But the curious *ornithic* nasal bones, deeply cleft in front, the imperfect desmognathism of the palate, the feeble and segmented state of the anterior sphenoid, and the open pituitary space of the embryonic cartilaginous skull, all these things suggest that the Pangolins, whatever degenerative specialisation they may have undergone, never did rise to any height, as Mammals.

Indeed, to me, their *pre-natal* development—the Eutherian placentation—seems to be their best title to be ranked even amongst the low forms of the high Mammalia.

If a complete series of fossil types could be found, on one hand stretching backwards (or downwards) from the Glyptodons, and on the other, from the Megatheroids, then long before these two groups merged into a common Prototherian root-stock, we should find their differences one by one dying out.

Embryology would help us here very much if materials could be obtained. Even with the scanty treasures that I have been able to obtain, most remarkable things are shown.

Of the two Anteaters I have only been able to obtain the young (not the embryo) of the smallest and most aberrant type—*Cycloturus*—and of the Sloths only two embryos, and one of these considerably advanced, belonging to two genera, namely, *Cholœpus* and *Bradypus* (*Arctopithecus*, Gray).

But every step backward in the structure of the skull of the Sloth brings me nearer and nearer to what I see even in the young of the Little Anteater; and that it is possible for both of these types to have arisen from the same stock is no longer a doubtful thing.

But the skull of developing embryos of the Sloth (of either kind) forms a very valuable and easy-working key to what is difficult in the skulls of the extinct gigantic Megatheroids.

If this be the case, if Sloths, extinct or recent, have arisen, during time, from the same stock as the great terrestrial Ant-bear, and the little prehensile-tailed *Cycloturus*, then there is nothing in any other Order to shock the mind or to be a stumbling block in the path of the most timid evolutionist.

That in the Armadillos the new husbandry, or growth, of hair—the correlate of milk glands—should thrive badly on the old stony ground of Reptilian horn-covered scales, breaking out where it can among the clefts, is not more wonderful than that this same new growth of hair in the Pangolin should mat itself together and imitate the scales of Reptiles and Fishes.

I have in the summary to my main paper mentioned several of the most important cranial characters in the Edentata, and have endeavoured to show their signification.

These, however, have to be studied with the help of the illustrations, and need not be spoken of now. Moreover, until the figures

and descriptions of the skull in the Insectivora can be brought out, the present paper, as a whole, is somewhat imperfect for want of standards for comparison, such as are to be found in the more normal skulls to be seen in the animals of that Order.

II. "On the Non-Euclidian Plane Geometry." By Professor CAYLEY, F.R.S. Received May 27, 1884.

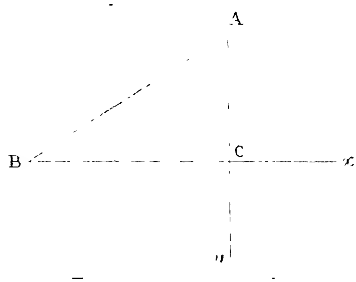
1. I consider the hyperbolic or Lobatschewskian geometry: this is a geometry such as that of the imaginary spherical surface $x^2+y^2+z^2=-1$; and the imaginary surface may be *bent* (without extension or contraction) into the real surface considered by Beltrami, and which I will call the Pseudosphere, viz., this is the surface of revolution defined by the equations $x=\log\cot\frac{1}{2}\theta-\cos\theta$, $\sqrt{y^2+z^2}=\sin\theta$. We have on the imaginary spherical surface imaginary points corresponding to real points of the pseudosphere, and imaginary lines (arcs of great circle) corresponding to real lines (geodesics) of the pseudosphere, and, moreover, any two such imaginary points or lines of the imaginary spherical surface have a real distance or inclination equal to the corresponding distance or inclination on the pseudosphere. Thus the geometry of the pseudosphere, using the expression straight line to denote a geodesic of the surface, is the Lobatschewskian geometry; or rather I would say this in regard to the metrical geometry, or trigonometry, of the surface; for in regard to the descriptive geometry, the statement requires (as will presently appear) some qualification.

2. I would remark that this realisation of the Lobatschewskian geometry sustains the opinion that Euclid's twelfth axiom is undemonstrable. We may imagine rational beings living in a two-dimensional space, and conceiving of space accordingly, that is having no conception of a third dimension of space; this two-dimensional space need not however be a plane, and taking it to be the pseudospherical surface, the geometry to which their experience would lead them would be the geometry of this surface, that is, the Lobatschewskian geometry. With regard to our own two-dimensional space, the plane, I have, in my Presidential Address (B.A., Southport, 1883) expressed the opinion that Euclid's twelfth axiom in Playfair's form of it does not need demonstration, but is part of our notion of space, of the physical space of our experience; the space, that is, which we become acquainted with by experience, but which is the representation lying at the foundation of all physical experience.

3. I propose in the present paper to further develope the geometry of the pseudosphere. In regard to the name, and the subject

generally, I refer to two memoirs by Beltrami, "Teoria fondamentale degli Spazii di Curvatura Costante," Annali di Matem., t. ii. (1868–69), pp. 232–255, and "Saggio di interpretazione della Geometria non-Euclidea," Giornale di Matem., t. vi (1868), pp. 284–312, both translated, Ann. de l'École Normale, t. vi (1869); in the last of these he speaks of surfaces of constant negative curvature as "pseudospherical," and in a later paper, "Sulla superficie di rotazione che serve di tipo alle superficie pseudosferiche," Gior. di Matem., t. x (1872), pp. 147–151, he treats of the particular surface which I have called the pseudosphere. The surface is mentioned, Note iv of Liouville's edition of Monge's "Application de l'Analyse à la Géometrie" (1850), and the generating curve is there spoken of as "bien connue des géomètres."

FIG. 1.



4. In ordinary plane geometry, take (fig. 1) a line Bx, and on it a point B; from B, in any direction, draw the line BA; take upon it a point A, and from this point, at right angles to Bx, draw Ay, cutting it at C. We have thus a triangle ACB, right-angled at C; and we may denote the other angles, and the lengths of the sides, by A, B, c, a, b, respectively. In the construction of the figure the length c and the angle B are arbitrary.

The plane is a surface which is homogeneous, isotropic, and palintropic. That is, whatever be the position of B, the direction of Bx, and the sense in which the angle B is measured, we have the same expressions for a, b as functions of c, B; these expressions, of course, are

$$a = c \cos B,\ b = c \sin B.$$

But considering Ay as the initial line, and AB$=c$, as a line drawn from A at an inclination thereto $=$A, we have in like manner

$$b = c \cos A,\ a = c \sin A,$$

and consequently $\cos A = \sin B$, $\sin A = \cos B$; whence $\sin (A+B) = 1$,

cos $(A+B)=0$, and thence $A+B=$ a right angle, or $A+B+C=$ two right angles.

Hence also in any triangle ABC, drawing a perpendicular, say AD from A to the side BC, and so dividing the triangle into two right-angled triangles, we prove that the sum $A+B+C$ of the angles is $=$ two right angles, and we further establish the relations

$$a=b\cos C+c\cos B,\ b=c\cos A+a\cos C,\ c=a\cos B+b\cos A,$$

which are the fundamental formulæ of plane trigonometry; that is, we derive the metrical geometry or trigonometry of the plane from the two original equations $a=c\cos B$, $b=c\sin B$.

5. Supposing the plane bent in any manner, converted that is into a developable surface or torse, and using the term straight line to denote a geodesic of the surface, then the straight line of the surface is in fact the form assumed, in consequence of the bending, by a straight line of the plane. The sides and angles of the rectilinear triangle ABC on the surface are equal to those of the rectilinear triangle ABC on the plane, and the metrical relations hold good without variation. But it is not *simpliciter* true that the descriptive properties of the torse are identical with those of the plane. This will be the case if the points of the plane and torse have with each other a (1, 1) correspondence, but not otherwise. For instance, consider a plane curve (such as the parabola or one branch of the hyperbola) extending from infinity to infinity, and let the torse be the cylinder having this curve for a plane section; then to each point of the plane their corresponds a single point of the cylinder; and conversely to each point of the cylinder there corresponds a single point of the plane; and the descriptive geometries are identical. In particular two straight lines (geodesics) on the cylinder cannot inclose a space; and Euclid's twelfth axiom holds good in regard to the straight lines (geodesics) of the cylinder. But take the plane curve to be a closed curve, or (to fix the ideas) a circle; the infinite plane is bent into a cylinder considered as composed of an infinity of convolutions; to each point of the plane there corresponds a single point of the cylinder, but to each point of the cylinder an infinity of points of the plane; and the descriptive properties are in this case altered; the straight lines (geodesics) of the cylinder are helices; and we can through two given points of the cylinder draw, not only one, but an infinity of helices; any two of these will inclose a space. And even if instead of the geodesics we consider only the shortest lines, or helices of greatest inclination; yet even here for a pair of points on opposite generating lines of the cylinder, there are two helices of equal inclination, that is, two shortest lines inclosing a space. We have in what precedes an illustration in regard to the descriptive geometry of the pseudosphere; this is not identical with

the Lobatschewskian geometry, but corresponds to it in a manner such as that in which the geometry of the surface of the circular cylinder corresponds to that of the plane.

6. The surface of the sphere is, like the plane, homogeneous, isotropic, and palintropic. We may on the spherical surface construct as above a right-angled triangle ABC, wherein the side c and the angle B are arbitrary; and (corresponding to the before-mentioned formulæ for the plane) we then have

$$\tan a = \tan c \cos B, \quad \sin b = \sin c \sin B,$$

whence also

$$\tan b = \tan c \cos A, \quad \sin a = \sin c \sin A.$$

We deduce from these

$$\frac{\tan^2 a}{\tan^2 c} + \frac{\sin^2 b}{\sin^2 c} = 1,$$

leading to $\cos^2 c = \cos^2 a \cos^2 b$; and then

$$\frac{\sin b}{\tan a} = \cos c \tan B, \quad \frac{\sin a}{\tan b} = \cos c \tan A,$$

giving $$\cos a \cos b = \cos^2 c \tan A \tan B;$$

that is $$\tan A \tan B = \frac{1}{\cos a \cos b}, \text{ which is} > 1.$$

Hence A+B > a right angle, or in the right-angled trangle ACB, the sum A+B+C of the angles is > two right angles. Whence also in any triangle ABC whatever, dividing it into two right-angled triangles by means of a perpendicular let fall from an angle on the opposite side, we have the sum A+B+C of the angles > two right angles. And we obtain, moreover,

$$a = \tan^{-1}(\tan c \cos B) + \tan^{-1}(\tan b \cos C),$$

$$b = \tan^{-1}(\tan a \cos C) + \tan^{-1}(\tan c \cos A),$$

$$c = \tan^{-1}(\tan b \cos A) + \tan^{-1}(\tan a \cos B),$$

which lead to all the formulæ of spherical trigonometry.

7. Suppose the radius of the sphere to be $1/\lambda$: then a, b, c being the lengths of the sides, the lengths in spherical measure are $\lambda a, \lambda b, \lambda c$; and we must in the formulæ instead of a, b, c write $\lambda a, \lambda b, \lambda c$ respectively. In particular for the imaginary sphere $x^2+y^2+z^2=-1$, we have $\lambda = i$, and we must instead of a, b, c write ai, bi, ci respectively. The fundamental formulæ for the right-angled triangle thus become

$$\tanh a = \tanh c \cos B, \quad \sinh b = \sinh c \sin B,$$

and these lead to all the trigonometrical formulæ, viz., any one of these is deduced from the corresponding formula of spherical trigonometry by writing therein *ai*, *bi*, *ci* for *a*, *b*, *c* respectively; or, what is the same thing, by changing the circular functions of the sides into the corresponding hyperbolic functions.

In particular for the right-angled triangle ACB we have

$$\tan A \tan B = \frac{1}{\cosh a \cosh b},$$

which for a and b real is <1, that is, $A+B<$ a right angle, or $A+B+C<$ two right angles, and thence also in any triangle whatever $A+B+C<$ two right angles. But the points A, B, C of any such triangle ABC on the imaginary sphere, and the lines BC, CA, AB which connect them, are imaginary: the meaning of the proof will better appear on passing to the pseudosphere.

8. We have to consider the imaginary spherical surface as *bent* into a real surface. This is, of course, an imaginary process, as any process must be which gives a transformation of imaginary points and lines into real points and lines; but the notion is not more difficult than that of the transformation of imaginary similarity, consisting in the substitution of ix, iy, iz for x, y, z respectively. We thus pass from imaginary points of the imaginary sphere $x^2+y^2+z^2=-1$ to real points of the real sphere $x^2+y^2+z^2=1$; or, again, from imaginary points of either of the real hyperboloids $x^2+y^2-z^2=-1$, $x^2+y^2-z^2=1$ to real points of the other of the same two real hyperboloids.

9. I consider the formulæ for the flexure of the imaginary sphere $X^2+Y^2+Z^2=-1$, into the pseudosphere $x=\log \cot\frac{1}{2}\theta-\cos\theta$, $\sqrt{y^2+z^2}=\sin\theta$: it would be allowable to dispense with Beltrami's subsidiary variables u, v, but I prefer to collect here all the formulæ. We have

$$X=\frac{-i}{\sqrt{1-u^2-v^2}}, \quad Y=\frac{u}{\sqrt{1-u^2-v^2}}, \quad Z=\frac{v}{\sqrt{1-u^2-v^2}},$$

values which give $X^2+Y^2+Z^2=-1$. And observe that taking u, v to be real magnitudes such that $u^2+v^2<1$, we have X a pure imaginary, but Y and Z each of them real. We consider on the imaginary sphere points having such coordinates X, Y, Z; any such point corresponds as will immediately appear to a real point on the pseudosphere, and (the distances and angles being the same for the pseudosphere as for the original imaginary spherical surface) it hence appears that (notwithstanding that the points on the imaginary spherical surface, and the lines joining such points, are imaginary) the distances and angles on the imaginary spherical surface are real.

$$\sin\theta=\frac{1-u}{\sqrt{1-u^2-v^2}},\qquad \phi=\frac{v}{1-u},$$

and thence

$$iX-Y=\sin\theta,\quad iX+Y=\sin\theta(\phi^2+\operatorname{cosec}^2\theta),\quad Z=\sin\theta\,.\,\phi.$$

Also

$$u=\frac{\phi^2-1+\operatorname{cosec}^2\theta}{\phi^2+1+\operatorname{cosec}^2\theta},\qquad v=\frac{2\phi}{\phi^2+1+\operatorname{cosec}^2\theta},$$

$$x=\log\cot\tfrac{1}{2}\theta+\cos\theta,\qquad y=\sin\theta\cos\phi,\qquad z=\sin\theta\sin\phi.$$

10. We have $dX^2+dY^2+dZ^2$ and $dx^2+dy^2+dz^2$ each $=\cot^2\theta d\theta^2+\sin^2\theta d\phi^2$. Writing P, $Q=iX-Y$, $iX+Y$ respectively, we in fact have $dX^2+dY^2+dZ^2=-dPdQ+dZ^2$, where P, Q, $Z=\sin\theta$, $\phi^2\sin\theta+\operatorname{cosec}\theta$, $\phi\sin\theta$ respectively, and thence

$$\begin{aligned}dZ&=\sin\theta\,d\phi+\phi\cos\theta d\theta,\\ dP&=\qquad\qquad\cos\theta d\theta,\\ dQ&=2\sin\theta\phi d\phi+(\phi^2\cos\theta-\operatorname{cosec}\theta\cot\theta)d\theta,\end{aligned}$$

giving the formula $dX^2+dY^2+dZ^2=\cot^2\theta d\theta^2+\sin^2\theta d\phi^2$; and then also $dx^2+dy^2+dz^2=dx^2+(d\sin\theta)^2+\sin^2\theta d\phi^2=(\cos^2\theta\cot^2\theta+\cos^2\theta)d\theta^2+\sin^2\theta d\phi^2=\cot^2\theta d\theta^2+\sin^2\theta d\phi^2$. Joining to these the differential expression in u, v we have

$$\begin{aligned}dX^2+dY^2+dZ^2&=\frac{(1-u^2-v^2)(du^2+dv^2)+(udu+vdv)^2}{(1-u^2-v^2)^2},\\ &=\cot^2\theta d\theta^2+\sin^2\theta d\phi^2,\\ &=dx^2+dy^2+dz^2,\end{aligned}$$

where the final equation $dX^2+dY^2+dZ^2=dx^2+dy^2+dz^2$, shows that the imaginary sphere $X^2+Y^2+Z^2=-1$ can be bent into the pseudosphere.

Observe that to given values of θ, ϕ there corresponds a single point on the pseudosphere, but not conversely, for if θ, ϕ be values corresponding to a given point, then corresponding to the same point we have θ, $\phi+n\pi$, where n is an arbitrary integer.

11. The geodesics of the imaginary spherical surface are, of course, its plane sections, any such section being determined by a linear equation $\alpha X+\beta Y+\gamma Z=0$, between the coordinates X, Y, Z. Since for a point corresponding to a real point of the pseudosphere X is a pure imaginary, while Y and Z are real, we see that for a geodesic corresponding to a real geodesic of the pseudosphere we must have α a pure imaginary, β and γ real; and, in fact, writing as above, $P=iX-Y$, $Q=iX+Y$, and therefore conversely $X=\tfrac{1}{2}i(-P-Q,$

$Y=\frac{1}{2}(-P+Q)$, the equation $\alpha X+\beta Y+\gamma Z=0$ becomes $(-\frac{1}{2}i\alpha-\frac{1}{2}\beta)P+(-\frac{1}{2}i\alpha+\frac{1}{2}\beta)Q+\gamma Z=0$, which will then be of the form $AP+BQ+CZ=0$, with real coefficients A, B, C : viz., we have P, Q, $Z=\sin\theta$, $\sin\theta(\phi^2+\operatorname{cosec}^2\theta)$, $\sin\theta\,.\,\phi$; and the equation thus is

$$A+B(\phi^2+\operatorname{cosec}^2\theta)+C\phi=0,$$

which is the equation for a geodesic (or straight line) on the pseudosphere. The equation $A+C\phi=0$, that is, $\phi=$const., is obviously that of a meridian.

12. If the geodesic pass through a given point θ_1, ϕ_1 we have, of course, $A+B(\phi_1{}^2+\operatorname{cosec}^2\theta_1)+C\phi_1=0$, and hence also the equation of a geodesic through the two points (θ_1, ϕ_1), (θ_2, ϕ_2) is

$$\begin{vmatrix} 1\ , & \phi^2+\operatorname{cosec}^2\theta\ , & \phi \\ 1\ , & \phi_1{}^2+\operatorname{cosec}^2\theta_1\ , & \phi_1 \\ 1\ , & \phi_2{}^2+\operatorname{cosec}^2\theta_2\ , & \phi_2 \end{vmatrix}=0.$$

We may for ϕ_1, ϕ_2 write $\phi_1+2n_1\pi$, $\phi_2+2n_2\pi$ respectively, n_1, n_2 being arbitrary integers; and it would thus at first sight appear that there could be drawn through the two points a doubly infinite series of geodesics. There is, in fact, a singly infinite system of geodesics: to show how this is, write for shortness Λ, Λ_1, Λ_2, α, α_1, α_2 for $\operatorname{cosec}^2\theta$, $\operatorname{cosec}^2\theta_1$, $\operatorname{cosec}^2\theta_2$, $2n\pi$, $2n_1\pi$, $2n_2\pi$ respectively; then the equation of the geodesic through the two points may be written

$$\begin{vmatrix} 1\ , & (\phi+\alpha)^2+\Lambda\ , & \phi+\alpha \\ 1\ , & (\phi_1+\alpha_1)^2+\Lambda_1\ , & \phi_1+\alpha_1 \\ 1\ , & (\phi_2+\alpha_2)^2+\Lambda_2\ , & \phi_2+\alpha_2 \end{vmatrix}=0,$$

where the constant $\alpha=2n\pi$ may be disposed of so as to simplify the formula as much as may be, it is what I have called an apoclastic constant. Taking β an arbitrary value, this may be transformed into

$$\begin{vmatrix} 1\ , & (\phi+\alpha+\beta)^2+\Lambda\ , & \phi+\alpha+\beta \\ 1\ , & (\phi_1+\alpha_1+\beta)^2+\Lambda_1\ , & \phi_1+\alpha_1+\beta \\ 1\ , & (\phi_2+\alpha_2+\beta)^2+\Lambda\ , & \phi_2+\alpha_2+\beta \end{vmatrix}=0,$$

and then assuming $\alpha=\alpha_1$, $\beta=-\alpha_1$, this becomes

$$\begin{vmatrix} 1\ , & \phi^2+\Lambda\ , & \phi \\ 1\ , & \phi_1{}^2+\Lambda_1\ , & \phi_1 \\ 1\ , & (\phi_2+\alpha_2-\alpha_1)^2+\Lambda_2\ , & \phi_2+\alpha_2-\alpha_1 \end{vmatrix}=0,$$

which is what the equation

$$\begin{vmatrix} 1, & \phi^2+\Lambda, & \phi \\ 1, & \phi_1^2+\Lambda_1, & \phi_1 \\ 1, & \phi_2^2+\Lambda_2, & \phi_2 \end{vmatrix}=0,$$

becomes on changing only ϕ_2 into $\phi_2+\alpha_2-\alpha_1$, that is, $\phi_2+2k_2\pi$, where k_2 is an arbitrary integer. We have thus through the two points a singly infinite series of geodesic lines; in general only one of these is a shortest line, but for points on opposite meridians there are two equal shortest lines.

13. For the distance between two points (θ_1, ϕ_1), (θ_2, ϕ_2) on the pseudosphere, taking (X_1, Y_1, Z_1), (X_2, Y_2, Z_2) for the corresponding points on the imaginary sphere, and writing as above P_1, $Q_1=iX_1-Y_1$, iX_1+Y_1; P_2, $Q_2=iX_2-Y_2$, iX_2+Y_2, we have

$$\begin{aligned}\cosh\delta &= -X_1X_2-Y_1Y_2-Z_1Z_2,\\ &=\tfrac{1}{2}(P_1Q_2+P_2Q_1)-Z_1Z_2,\\ &=\sin\theta_1\sin\theta_2\{\tfrac{1}{2}(\phi_1^2+\operatorname{cosec}^2\theta_1)+\tfrac{1}{2}(\phi_2^2+\operatorname{cosec}^2\theta_2)-\phi_1\phi_2\},\\ &=\tfrac{1}{2}\sin\theta_1\sin\theta_2(\phi_2-\phi_1)^2+1+\frac{\tfrac{1}{2}(\sin\theta_2-\sin\theta_1)^2}{\sin\theta_1\sin\theta_2}.\end{aligned}$$

Observe here that writing θ_2, $\phi_2=\theta_1+d\theta_1$, $\phi_1+d\phi_1$, and therefore δ small so that $\cosh\delta=1+\frac{1}{2}\delta^2$, we obtain

$$\delta^2=\sin^2\theta_1 d\phi_1^2+\cot^2\theta_1 d\theta_1^2,$$

agreeing with the expression for $dx^2+dy^2+dz^2$. If in the form first obtained we write $\Lambda_1=\operatorname{cosec}^2\theta_1$, $\Lambda_2=\operatorname{cosec}^2\theta_2$, we find

$$\cosh\delta=\frac{\frac{1}{2}}{\sqrt{\Lambda_1\Lambda_2}}\{\phi_1^2+\Lambda_1+\phi_2^2+\Lambda_2-2\phi_1\phi_2\},$$

which is a convenient form.

In like manner, to find the mutual inclination of the two geodesics

$$A_1+B_1(\phi^2+\operatorname{cosec}^2\theta)+C_1\phi=0,$$
$$A_2+B_2(\phi^2+\operatorname{cosec}^2\theta)+C_2\phi=0,$$

these correspond to the plane sections $A_1P+B_1Q+C_1Z=0$, $A_2P+B_2Q+C_2Z=0$, that is $(A_1+B_1)iX+(-A_1+B_1)Y+C_1Z=0$, $(A_2+B_2)iX_2+(-A_2+B_2)Y+C_2Z=0$, of the imaginary sphere: and we thence find

$$\cos\Omega=\frac{C_1C_2-2(A_1B_2+A_2B_1)}{\sqrt{C_1^2-4A_1B_1}\sqrt{C_2^2-4A_2B_2}}.$$

15. Suppose that the two geodesics meet in the point θ_0, ϕ_0: then

writing for shortness $\operatorname{cosec}^2\theta = \Lambda$, and therefore $\operatorname{cosec}^2\theta_0 = \Lambda_0$, we have

$$A_1 + B_1(\phi_0^2 + \Lambda_0) + C_1\phi_0 = 0,$$

$$A_2 + B_2(\phi_0^2 + \Lambda_0) + C_2\phi_0 = 0.$$

Suppose that the meridian through this point is

$$A_3 + B_3(\phi^2 + \Lambda) + C_3\phi = 0,$$

then $B_3 = 0$, $A_3 + C_3\phi_0 = 0$. Take Ω_1, Ω_2, for the inclinations to this meridian of the two geodesics respectively, then

$$\cos\Omega_1 = \frac{C_1C_3 - 2A_3B_1}{\sqrt{C_1^2 - 4A_1B_1}\,.\,C_3} = \frac{C_1 + 2B_1\phi_0}{\sqrt{C_1^2 - 4A_1B_1}},$$

whence $$\sin\Omega_1 = \frac{2B_1\sqrt{\Lambda_0}}{\sqrt{C_1^2 - 4A_1B_1}},$$

and similarly $$\cos\Omega_2 = \frac{C_2 + 2B_2\phi_0}{\sqrt{C_2^2 - 4A_2B_2}},$$

whence $$\sin\Omega_2 = \frac{2B_2\sqrt{\Lambda_0}}{\sqrt{C_2^2 - 4A_2B_2}}.$$

We thence obtain

$$\cos(\Omega_1 - \Omega_2) = \frac{C_1C_2 + 2\phi_0(B_1C_2 + B_2C_1) + 4B_1B_2(\phi_0^2 + \Lambda_0)}{\sqrt{C_1^2 - 4_1A_1B}\sqrt{C_2^2 - 4A_2B_2}},$$

which is

$$= \frac{C_1C_2 - 2(A_1B_2 + A_2B_1)}{\sqrt{C_1^2 - 4A_1B_1}\sqrt{C_2^2 - 4A_2B_2}} = \cos\Omega, \text{ as above,}$$

the equality of the two numerators depending on the identity

$$(A_1 + B_1(\phi_0^2 + \Lambda_0) + C_1\phi_0)B_2 + (A_2 + B_2(\phi_0^2 + \Lambda_0) + C_2\phi_0)B_1 = 0.$$

In particular, if we consider the two geodesics

$$\phi^2 + \operatorname{cosec}^2\theta - \operatorname{cosec}^2\theta_1 + C_1\phi = 0, \qquad \phi = 0,$$

the second of which may be considered as representing any meridian section of the pseudosphere, and the first is an arbitrary geodesic meeting this at the point $\theta = \theta_1$, $\phi = 0$, then the formula for the inclination is

$$\cos\Omega = \frac{C_1}{\sqrt{C_1^2 + 4\operatorname{cosec}^2\theta_1}}.$$

Hence also, $\cos\Omega = 0$, or $\Omega = 90°$, if $C_1 = 0$: viz. we have $\phi^2 + \operatorname{cosec}^2\theta - \operatorname{cosec}^2\theta = 0$ for the equation of the geodesic through the point $\theta = \theta_1$, $\phi = 0$, at right angles to the meridian section $\phi = 0$.

16. Consider a right-angled triangle ACB, where the points A, C are on the meridian $\phi=0$, and write $(\theta_1, 0; \Lambda_1=\operatorname{cosec}^2\theta_1)$, $(\theta_2, \phi_2; \Lambda_2=\operatorname{cosec}^2\theta_2)$ $(\theta_3, 0; \Lambda_3=\operatorname{cosec}^2\theta_3)$ for the points A, B, C, respectively. Then if equations are—

for side BC, $A_1+B_1(\phi^2+\Lambda)+C_1\phi=0$, we have $C_1=0$,

$A_1+B_1(\phi_2^2+\Lambda_2)=0$, $A_1+B_1\Lambda_3=0$, whence $\phi_2^2+\Lambda_2=\Lambda_3$;

for side CA, $A_2+B_2(\phi^2+\Lambda)+C_2\phi=0$, we have $A_2=0$, $B_2=0$;

for side AB, $A_3+B_3(\phi^2+\Lambda)+C_3\phi=0$,

we have $A_3+B_3(\phi_2^2+\Lambda_2)+C_3\phi_2=0$, $A_3+B_3\Lambda_1=0$.

Observing that $\phi_1=\phi_3=0$, we have

$$\cosh a=\frac{1}{2\sqrt{\Lambda_2\Lambda_3}}(\phi_2^2+\Lambda_2+\Lambda_3),$$

$$\cosh b=\frac{1}{2\sqrt{\Lambda_1\Lambda_3}}(\Lambda_3+\Lambda_1),$$

$$\cosh c=\frac{1}{2\sqrt{\Lambda_1\Lambda_2}}(\phi_2^2+\Lambda_1+\Lambda_2);$$

or, reducing these by the relation $\phi_2^2+\Lambda_2=\Lambda_3$, they become

$$\cosh a=\frac{\sqrt{\Lambda_3}}{\sqrt{\Lambda_2}},\quad \text{whence } \sinh a=\frac{\sqrt{\Lambda_3-\Lambda_2}}{\sqrt{\Lambda_2}},\quad \tanh a=\frac{\sqrt{\Lambda_3-\Lambda_2}}{\sqrt{\Lambda_3}};$$

$$\cosh b=\frac{\Lambda_1+\Lambda_3}{2\sqrt{\Lambda_1\Lambda_3}},\quad ,,\ \sinh b=\frac{\Lambda_1-\Lambda_3}{2\sqrt{\Lambda_1\Lambda_3}},\quad \tanh b=\frac{\Lambda_1-\Lambda_3}{\Lambda_1+\Lambda_3};$$

$$\cosh c=\frac{\Lambda_1+\Lambda_3}{2\sqrt{\Lambda_1\Lambda_2}},\quad ,,\ \sinh c=\frac{\sqrt{(\Lambda_1+\Lambda_3)^2-4\Lambda_1\Lambda_2}}{2\sqrt{\Lambda_1\Lambda_2}},$$

$$\tanh c=\frac{\sqrt{(\Lambda_1+\Lambda_3)^2-4\Lambda_1\Lambda_2}}{\Lambda_1+\Lambda_3}.$$

We have, moreover,

$$\cos B=\frac{C_1C_3-2(A_1B_3+A_3B_1)}{\sqrt{C_1^2-4A_1B_1}\sqrt{C_3^2-4A_3B_3}}=\frac{-2(A_1B_3+A_3B_1)}{\sqrt{-4A_1B_1}\sqrt{C_3^2-4A_3B_3}},$$

which, writing $A_3=-B_3\Lambda_1$, becomes

$$\cos B=\frac{B_3(\Lambda_1+\Lambda_3)}{\sqrt{\Lambda_3}\sqrt{C_3^2-4A_3B_3}};$$

Or further reducing by means of

$$\phi_2^2(C_3^2-4A_3B_3)=B_3^2(\phi_2^2+\Lambda_2-\Lambda_1)^2+4\phi_2^2B_3^2\Lambda_1^2$$

$$=B_3^2\{(\phi_2^2+\Lambda_2-\Lambda_1)^2+4\phi_2^2\Lambda_1\}$$

$$=B_3^2\{(\Lambda_3+\Lambda_1)^2+4\Lambda_1(\Lambda_3-\Lambda_2)\}$$

$$=B_3^2\{(\Lambda_3+\Lambda_1)^2-4\Lambda_1\Lambda_2\},$$

this becomes
$$\cos B=\frac{(\Lambda_1+\Lambda_3)\sqrt{\Lambda_3-\Lambda_2}}{\sqrt{\Lambda_3}\sqrt{(\Lambda_1+\Lambda_3)^2-4\Lambda_1\Lambda_2}},$$

whence
$$\sin B=\frac{\sqrt{\Lambda_2}(\Lambda_1-\Lambda_3)}{\sqrt{\Lambda^3}\sqrt{(\Lambda_1+\Lambda_3)^2-4\Lambda_1\Lambda_2}};$$

and with these values we verify

$$\tanh a=\tanh c\,.\cos B,\qquad \sinh b=\sinh c\,.\sin B,$$

which are the expressions for the sides BC, CA, in terms of the length BA, $=c$ and angle B, which are arbitrary. I have not thought it necessary to give the direct verification of these equations for a more general position of the right-angled triangle: we already know, and it appears *à posteriori* by the following number, that the verification really extends to any right-angled triangle whatever on the surface.

17. The pseudosphere is homogeneous, isotropic, and palintropic, viz., this is the case when bending is allowed; in other words, the surface is applicable upon itself, with three degrees of freedom; considering any infinitesimal linear element Ax, the point A may be brought to coincide with an arbitrary point A′ of the surface, with the element Ax in an arbitrary direction $A'x'$ through A′, and the area about A will then coincide with the area about A′. The analytical theory is at once derived from that for the sphere, viz., we have a rectangular transformation

	X	Y	Z
X_1	α	β	γ
Y_1	α'	β	γ'
Z_1	α''	β''	γ''

where the coefficients are such that identically

$$X_1^2+Y_1^2+Z_1^2=X^2+Y^2+Z^2;$$

and where the coefficients are connected by six equations only, the system thus depending on three arbitrary parameters. If, as before, we write P_1, Q_1, P, Q, for iX_1-Y_1, iX_1+Y_1, $iX-Y$, $iX+Y$ respectively, then the relation is readily found to be

	P	Q	Z
P_1	$\frac{1}{2}(\alpha+i\alpha'-i\beta+\beta')$	$\frac{1}{2}(\alpha+i\alpha'+i\beta-\beta')$	$i\gamma-\gamma'$
Q_1	$\frac{1}{2}(\alpha-i\alpha'-i\beta-\beta')$	$\frac{1}{2}(\alpha-i\alpha'+i\beta+i\beta')$	$i\gamma+\gamma'$
Z_1	$\frac{1}{2}(-i\alpha''-\beta'')$	$\frac{1}{2}(-i\alpha''+\beta'')$	γ''

this being read according to the lines only $P_1=\frac{1}{2}(\alpha+i\alpha'-i\beta+\beta')P+$ &c., not according to the columns: in order that the coefficients may be real, we must have α, β', γ', β'', γ'', real, β, γ, α', α'' pure imaginaries.

Writing the equations in the form

	P	Q	Z
P_1	A	B	C
Q_1	A′	B′	C′
Z_1	A″	B″	C″

viz., $P_1=AP+BQ+CZ$, &c.
it would be possible to deduce the equations which connect the new coefficients; but these are more easily obtained from the consideration that we must have identically $P_1Q_1-Z_1^2=PQ-Z^2$; the equations are thus found to be

$$A''^2-AA'=0,\qquad B''^2-BB'=0,\qquad C''^2-CC'=1$$

$$2A''B''-AB'-A'B=-1,\qquad 2A''C''-AC'-A'C=0,$$

$$2B''C''-BC'-B'C=0.$$

18. The general theory of the transformation of a quadric function into itself enables us to express the coefficients in terms of three arbitrary parameters. There is no difficulty in working out the formulæ, and we finally obtain

$$\Omega P_1 = -(\nu+1)^2 P - \lambda^2 Q + 2\lambda(\nu+1)Z,$$
$$\Omega Q_1 = -\mu^2 P - (\nu-1)^2 Q + 2\mu(\nu-1)Z,$$
$$\Omega Z_1 = -\mu(\nu+1)P - \lambda(\nu-1)Q + (-1+\nu^2+\lambda\mu)Z;$$

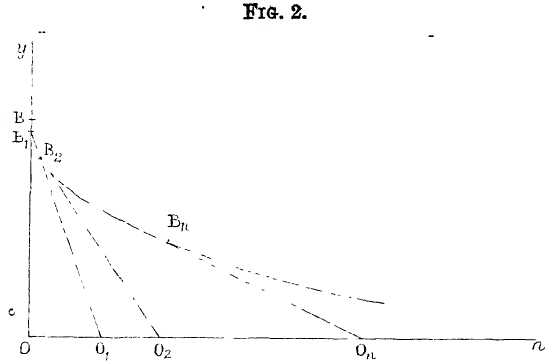
and conversely

$$\Omega P = -(\nu-1)^2 P_1 - \lambda^2 Q_1 + 2\lambda(\nu-1)Z_1,$$
$$\Omega Q = -\mu^2 P_1 - (\nu+1)^2 Q_1 + 2\mu(\nu+1)Z_1,$$
$$\Omega Z = -\mu(\nu-1)P_1 - \lambda(\nu+1)Q_1 + (1+\nu^2+\lambda\mu)Z_1,$$

where $\Omega = -1+\nu^2-\lambda\mu$: it can be at once verified that each of the two sets of formulæ does in fact give $P_1Q_1 - Z_1^2 = PQ - Z^2$.

19. The pseudosphere is a surface of revolution having for its meridian section the curve $x = \log\cot\frac{1}{2}\theta - \cos\theta$, $y = \sin\theta$. This is a curve symmetrical in regard to the axis of y; and we obtain the portion of it lying on the positive side of this axis, by giving to θ the series of values $\theta = 0$ to $\theta = 90°$; for $\theta = 0$, we have $y = 0$, $x = \infty$, or the axis of x is an asymptote; for $\theta = 90°$, $x = 0$, $y = 1$, the point being a cusp of the curve. The geometrical definition is that the portion of the tangent included between the curve and the axis of x has the constant length $= 1$; the inclination of the tangent is in fact $= \theta$. We have $dx = \frac{\cos^2\theta d\theta}{\sin\theta}$, $dy = \cos\theta d\theta$; and thence $ds = \cot\theta d\theta$, and the

Fig. 2.



length in question is $\frac{y\,ds}{dy} = 1$. The curve may be constructed graphically: take (fig. 2) the distance $BO = 1$, on OB, B_1 very near to B, and then $B_1O_1 = 1$; on O_1B_1, B_2 very near to B_1, and then $B_2O_2 = 1$, and so on; the curve is shown on a larger scale in fig. 3.

But the curve may also be laid down numerically; writing $\alpha = \frac{1}{2}\pi - \theta$ (so that α is the inclination of the tangent to the axis of y)

we have $x=\log\tan(\frac{1}{4}\pi+\frac{1}{2}\alpha)-\sin\alpha$, $y=\cos\alpha$, where $\log\tan(\frac{1}{4}\pi+\frac{1}{2}\alpha)$, the hyperbolic logarithm (which has been the signification of log throughout), is the function tabulated Tab. IV. Leg. "Traité des Fonctions Elliptiques," t. ii, pp. 256—259.

We may hence obtain the values of the coordinates as follows:—

$\alpha=90^\circ-\theta$	$\log\tan(\frac{1}{4}\pi+\frac{1}{2}\alpha)$	$-\sin\alpha$	x	$y=\cos\alpha$
0	0·0000000	−0·0000000	0·0000000	1·0000000
10°	0·1754258	0·1736482	0·0017776	0·9848078
20°	0·3563785	0·3420201	0·0143584	0·9396926
30°	0·5493061	0·5000000	0·0493061	0·8660254
40°	0·7629096	0·6427876	0·1201220	0·7660444
50°	1·0106831	0·7660444	0·2446387	0·6427876
60	1·3169578	0·8660254	0·4509324	0·5000000
70°	1·7354151	0·9396926	0·7957225	0·3563785
80°	2·4362460	0·9848078	1·4514382	0·1754258
85°	3·1313013	0·9961947	2·1351066	0·0871557
86°	3·3546735	0·9975641	2·3571019	0·0697565
87°	3·6425333	0·9986295	2·6439038	0·0523360
88°	4·0481254	0·9993908	3·0487346	0·0348995
89°	4·7413487	0·9998477	3·7415010	0·0174524
90°	∞	−1·0000000	∞	0·0000000

Attending only to one-half of the surface we may regard the surface as standing on the circular base $y^2+z^2=1$: say this circle is the equator, or the unit-circle: the horizontal section being always a circle, the radius diminishing at first rapidly and then more and more slowly from 1 to 0 as the height increases from 0 to ∞. It is hardly necessary to remark that the radius of the equator is any given length whatever, taken as unity: the equations might, of course, have been written $x=c\{\log\cot\frac{1}{2}\theta-\cos\theta\}$, $\sqrt{y^2+z^2}=c\sin\theta$, but there would have been no gain of generality in this.

20. The geodesics are as already seen given by an equation $A+B(\phi^2+\operatorname{cosec}^2\theta)+C\phi=0$. If $B=0$, we have $A+C\phi=0$, that is $\phi=$const., which belongs to the meridians; if B be not$=0$, we may by a mere change of ϕ, that is, of the initial meridian, reduce the form to $A+B(\phi^2+\operatorname{cosec}^2\theta)=0$, which is the equation of a geodesic cutting at right angles the meridian $\phi=0$; writing herein $\sin\theta=\frac{1}{r}$, we have $A+B\left(\phi^2+\frac{1}{r^2}\right)=0$, which is the equation in the polar coordinates r, ϕ of the projection of the geodesic on the equatorial plane $x=0$: putting herein for greater convenience $B=-Ak^2$, we have $r^2=\frac{k^2}{1-k^2\phi^2}$: we require only such portions of the curves as lie within the unit-circle, and need therefore attend only to those for which k is

not greater than 1, and in any such curve consider ϕ as extending from $\phi=0$ to $\phi=\pm\frac{\sqrt{1-k^2}}{k}$: writing this last value $=\pm\gamma$, we have $k=\frac{1}{\sqrt{1+\gamma^2}}$; if $\gamma<\pi$, that is, $k<\frac{1}{\sqrt{1+\pi^2}}$, the curve is a mere arc cutting at right angles (at the distance $r=k$ from the centre) the meridian $\phi=0$, and extending itself out on each side to meet the unit-circle in the points $\phi=\gamma$, $\phi=-\gamma$ respectively; in the case $\gamma=\pi$, that is, $k=\frac{1}{\sqrt{1+\pi^2}}$, the two points $\phi=\pm\gamma$ come together at the point $\phi=\pi$, or the curve becomes a loop; and for larger values, $k=\frac{1}{\sqrt{1+\pi^2}}$ to $\frac{1}{\sqrt{1+4\pi^2}}$, we have the two branches crossing each other on the meridian $\phi=\pi$ at the distance $r=\frac{k}{\sqrt{1-k^2\pi^2}}$ from the centre and then extending themselves in the opposite semicircles, so as to meet the unit-circle at the points $\phi=\pm\gamma$. And we have thus another critical value $k=\frac{1}{\sqrt{1+4\pi^2}}$, for which the two branches having thus crossed each other come to unite themselves at the point $\phi(=2\pi)=0$ of the unit-circle; and in like manner the critical values $\frac{1}{\sqrt{1+9\pi^2}}$, $\frac{1}{\sqrt{1+16\pi^2}}$, &c.: for a value of k between such limits the branch is a spiral having a determinate number of convolutions, and the two branches cross each other always on the radii $\phi=0$ and $\phi=\pi$ respectively.

21. Let ψ denote the inclination of the radius vector to the normal, or what is the same thing, that of the element of the circular arc to the tangent; we have $\tan\psi=\frac{dr}{rd\phi}$, and $\frac{dr}{rd\phi}=\frac{k^2\phi}{1-k^2\phi^2},=r^2\phi$, that is, $\tan\psi=r^2$. At the intersection with the unit section $r=1$, and therefore $\tan\psi=\phi$; moreover putting $k=\cos\kappa$, so that the equation of the curve now is $r^2=\frac{\cos^2\kappa}{1-\phi^2\cos^2\kappa}$, then for $r=1$ we have $\phi=\tan\kappa$; and hence at the intersection with the unit-circle $\psi=\kappa$; that is as k decreases from $k=1$, or k increases from $k=0$, the angle at which each curve cuts the unit-circle is always $=\kappa$, and thus this angle continually increases from $\kappa=0$; for $k=\frac{1}{\sqrt{1+\pi^2}}=\cos\kappa$, and therefore $\tan\kappa=\pi$, we have $\kappa=72^\circ\,20'$ nearly, the complement hereof $17^\circ\,40'$ is

thus the angle at which each branch of the loop cuts the meridian $\phi=\pi$.

22. To obtain another datum convenient in tracing the curve, I write $\phi=\phi_0=\tan\kappa$ for the value of ϕ at the unit-circle; and introducing for the moment the rectangular coordinates $X=r\sin\phi$, $Y=1-r\cos\phi$, then we easily find $\dfrac{dY}{dX}=\dfrac{r\sin\phi-r^3\phi\cos\phi}{r\cos\phi+r_3\phi\sin\phi}$; and thence for the equation of the tangent at the point on the unit-circle,

$$(y-1+\cos\phi_0)=\frac{\sin\phi_0-\phi_0\cos\phi_0}{\cos\phi_0+\phi_0\sin\phi_0}(x-\sin\phi_0).$$

For the tangent at the point of intersection with the radius $\phi=0$, or say the apse, we have $y=1-\cos\kappa$; and hence at the intersection of the two tangents

$$x=\sin\phi_0+\frac{\cos\phi_0+\phi_0\sin\phi_0}{\sin\phi_0-\phi_0\cos\phi_0}(\cos\phi_0-\cos\kappa)$$

$$=\frac{1-\cos\kappa(\cos\phi_0+\phi_0\sin\phi_0)}{\sin\phi_0-\phi_0\cos\phi_0},$$

which putting therein $\phi_0=\tan\kappa$ becomes

$$=\frac{\cos\kappa\{1-\cos(\phi_0-\kappa)\}}{\sin(\phi_0-k)}=\cos\kappa\tan\tfrac{1}{2}(\phi_0-\kappa),$$

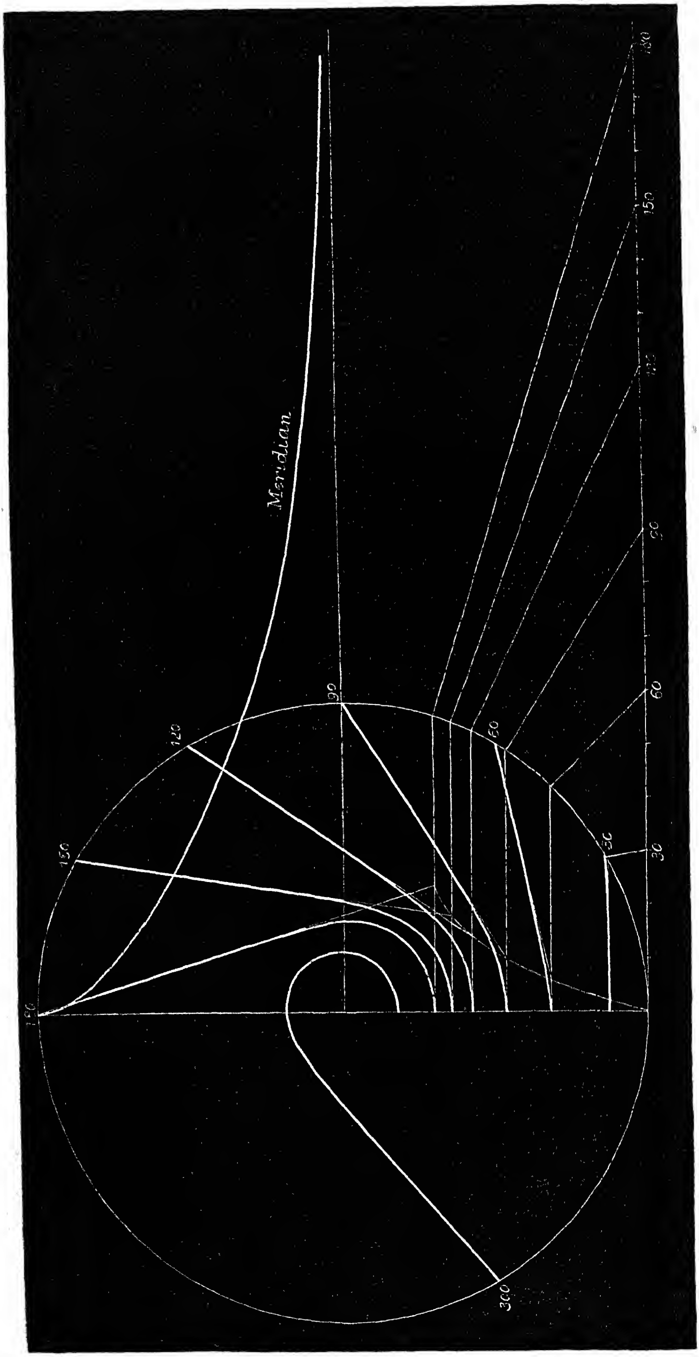
where ϕ_0 is given in terms of κ by the just mentioned equation $\phi_0=\tan\kappa$. We have $y=1-\cos\kappa$, $x=\cos\kappa\tan\frac{1}{2}(\phi_0-\kappa)$ for the locus of the intersection of the two tangents; this is easily seen to be a curve having a cusp at the unit-circle.

23. Fig. 3 shows the curves for the values

$\phi_0=$	$\tan\kappa$.	$\kappa=$
$30^\circ=\frac{1}{6}\pi$	0·5235988	27° 38′
60	1·0471976	46 19
90	1·5707963	57 31
120	2·0943941	64 29
150	2·6179939	69 5
$180=\pi$	3·1415926	72 20

We construct and graduate the unit-circle; draw to it a tangent at 0°, and measuring off from 0 a distance equal to the semi-circumference, graduate this in like manner in equal parts 0° to 180°; then to find the curve belonging, for instance, to $\phi_0=90^\circ$, we join with the centre of the circle the point 90° of the tangent, thus deter-

Fig. 3.



mining on the unit-circle a point belonging to the angle $\kappa=57° 31'$; at this point we draw parallel to the tangent a line which is the tangent at the lowest point; the curve passes through the point 90° on the unit-circle, and there cuts the circle at the angle $\kappa=57° 31'$ (or, what is the same thing, the radius at the complementary angle), and we have thus the tangent at the point 90° of the unit-circle; it will be noticed that this meets the tangent at the apse at a point near to this apse, so that the arc as determined by the two tangents is for a large part of its course nearly a right line; this is still more the case for smaller values of ϕ_0 or κ, while for larger values the deviation increases, but in the neighbourhood of the unit-circle the form is always nearly rectilinear.

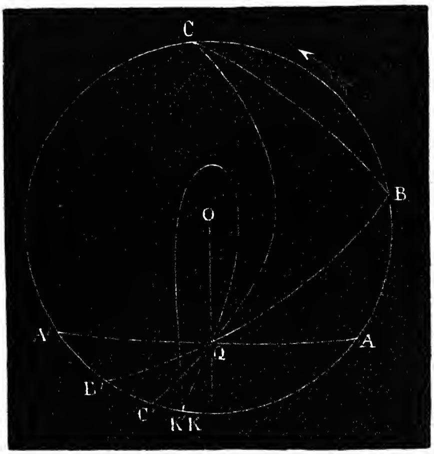
I show in the same figure the form of the curve for $\phi_0=300°$, $=5{\cdot}2359877$, $=\tan\kappa$, that is $\kappa=79° 11'$, $r=\cos\kappa=0{\cdot}1876670$, the value at the apse: the construction for the tangent at the unit-circle is the same as before, but in order to lay down the curve with tolerable accuracy we require also the value of r at the node on the meridian $\phi=180°$; this is, of course, given by $r=\dfrac{\cos\kappa}{\sqrt{1-\pi^2\cos^2\kappa}}$, that is $r=\dfrac{\cos\kappa}{\cos\alpha}$, if $\pi\cos\kappa=\sin\alpha$; whence without difficulty $r=0{\cdot}23236$, the value at the node.

24. The curves shown in the figure are projections upon the plane of the unit-circle, viz., they are the projections on this plane of the geodesics, which cut at right angles a given meridian; but bearing in mind the form of the meridian, it is easy, by means of the projection, to understand the actual forms on the surface of the pseudosphere. A point near the centre of the figure represents a point high up on the surface; and in any radius the portions near the centre are the more foreshortened in the figure, and represent greater distances on the surface. Each geodesic cutting the meridian at right angles at the apse descends symmetrically on the two sides, reaches ultimately—it may be after many convolutions—the unit-circle; the meridian itself is a limiting or special form of geodesic. The unit-circle is not properly a geodesic, but it is an envelope of geodesics.

25. To obtain all the geodesics we have to consider the geodesics which cut at right angles a given meridian; and then to imagine this meridian (with the geodesics which belong to it) turned round so as to occupy successively the positions of all the other meridians. The same remark applies of course to the projections; the figure shows the projections cutting at right angles a given radius of the circle; and this radius (with the projections belonging to it) is then to be turned round so as to occupy successively the positions of all the other radii. We may imagine the several geodesics turned round separately, each through a different angle, so as to bring each of

them to pass through one and the same point of the surface; we have then the geodesics drawn in all directions through this point of the surface; doing the same thing with the projections, we have, it is clear, the projections of the geodesics drawn in all directions through the point. It is easy, by drawing the projections each on a separate circle of paper, and passing a pin through the centres, to form a model by means of which an accurate figure of the projection may be constructed. But I content myself with a mere diagram (fig. 4).

FIG. 4.



26. Taking a point Q so low down on the surface that the geodesic at right angles to the meridian through Q is a simple arc A′A, then imagining the two extremities A, A′ each moving in the same sense round the circle, but A faster than A′, so as to assume the positions B, B′; C, C′; and so on to K, K′ coinciding with each other, we have the arcs B′B, C′C, and so on until we come to the loop form K′K: after which we have L′ in advance of L, and so on to curves of any number of convolutions. Considering any two arcs—B′B, C′C—and drawing the geodesic BC which joins their extremities B and C, then any geodesic through Q intermediate to B′B, C′C, or, say, to QB, QC, will meet the arc BC; while the geodesics through Q extramediate to QB, QC *will not meet, or will only after a convolution or convolutions meet,* the arc BC. This of course corresponds to the Lobatschewskian theory, according to which we have through a point Q to the extremities at infinity of a line BC, two distinct lines QB, QC, said to be the parallels through Q of the line BC; and which are such that any line through Q intermediate to

QB, QC meets the line BC; while any line through Q extramediate to QB, QC *does not meet* the line BC.

27. It is interesting to connect the theory of the geodesics of the pseudosphere with the general theory of geodesics. Starting with the form

$$ds^2=\cot^2\theta d\theta^2+\sin^2\theta d\phi^2, =\mathrm{E}d\theta^2+2\mathrm{F}d\phi d\phi+\mathrm{G}d\phi^2,$$

we have $\mathrm{E}=\cot^2\theta$, $\mathrm{F}=0$, $\mathrm{G}=\sin^2\theta$; and therefore $\mathrm{E}+1=\frac{1}{\mathrm{G}}$, or $\mathrm{E}=\frac{1}{\mathrm{G}}-1$, and the differential equation of the geodesic becomes

$$\mathrm{E}\theta'\,.\,2\mathrm{G}_1\theta'\phi'-\mathrm{G}\phi'(\mathrm{E}_1\theta'^2-\mathrm{G}_1\phi'^2)+2\mathrm{EG}(\theta'\phi''-\theta''\phi')=0;$$

that is

$$\phi'[(2\mathrm{EG}_1-\mathrm{GE}_1)\theta'^2+\mathrm{GG}_1\phi'^2]+2\mathrm{EG}(\theta'\phi''-\theta''\phi')=0,$$

where $$\mathrm{E}_1=\frac{d\mathrm{E}}{d\theta},\ \mathrm{G}_1=\frac{d\mathrm{G}}{d\theta},$$

and writing here $$\mathrm{E}=\frac{1}{\mathrm{G}}-1,$$

we have $$\mathrm{E}_1=-\frac{\mathrm{G}_1}{\mathrm{G}^2},\quad 2\mathrm{EG}_1-\mathrm{E}_1\mathrm{G}=\mathrm{G}_1\left(\frac{3}{\mathrm{G}}-2\right).$$

Moreover, from $\mathrm{G}=\sin^2\theta$ we find $\mathrm{G}_1=2\sqrt{\mathrm{G}\,.\,1-\mathrm{G}}$, and the equation becomes

$$\frac{\sqrt{\mathrm{G}}}{\sqrt{1-\mathrm{G}}}\left[\left(\frac{3}{\mathrm{G}}-2\right)\theta'^2+\mathrm{G}\phi'^2\right]\phi'+\theta'\phi''-\theta''\phi'=0.$$

Introducing here G in place of θ by the equation $\mathrm{G}=\sin^2\theta$, we have

$$\theta'=\frac{\mathrm{G}'}{2\sqrt{\mathrm{G}\,.\,1-\mathrm{G}}}.$$

$$\theta''=\frac{1}{4\mathrm{G}^{\frac{3}{2}}(1-\mathrm{G})^{\frac{3}{2}}}\{2\mathrm{G}(1-\mathrm{G})\mathrm{G}''-\mathrm{G}'^2(1-2\mathrm{G})\},$$

and the equation thus becomes

$$\begin{aligned}&(3-2\mathrm{G})\mathrm{G}'^2\phi'+4\mathrm{G}^3(1-\mathrm{G})\phi'^3\\&+2\mathrm{G}(1-\mathrm{G})\mathrm{G}'\phi''+\{-2\mathrm{G}(1-\mathrm{G})\mathrm{G}''+(1-2\mathrm{G})\mathrm{G}'^2\}\phi'=0.\end{aligned}$$

The whole term in ϕ' is thus $\phi'\{-2\mathrm{G}(1-\mathrm{G})\mathrm{G}''+(4-4\mathrm{G})\mathrm{G}'^2\}$, which divides by $2(1-\mathrm{G})$; the whole equation thus divides by $2(1-\mathrm{G})$, and omitting this factor, the equation becomes

$$\phi'(2\mathrm{G}'^2-\mathrm{GG}'')+2\phi'^3\mathrm{G}^3+\phi''\mathrm{GG}'=0,$$

which is simplified by introducing $H=\sin^2\theta=\frac{1}{G}$, instead of G. We in fact have $G'=\frac{-H'}{H^2}$, $G''=\frac{-H''}{H^2}+\frac{2H'^2}{H^3}$, and substituting these values, the equation becomes

$$\phi'\left(\frac{2H'^2}{H^4}+\frac{H''}{H^3}-\frac{2H'^2}{H^4}\right)+\frac{2}{H^3}\phi'^3-\frac{H'}{H^3}\phi''=0\,;$$

viz., this is $$H''\phi'+2\phi'^3-H'\phi''=0.$$

Writing herein $\phi+\alpha=\sqrt{K}$ (α an arbitrary constant) we have

$$\phi'=\frac{\frac{1}{2}K'}{\sqrt{K}},\qquad \phi''=\frac{\frac{1}{2}K''}{\sqrt{K}}-\frac{\frac{1}{4}K'^2}{K\sqrt{K}}$$

and the equation becomes

$$H''\frac{\frac{1}{2}K'}{\sqrt{K}}+\frac{\frac{1}{4}K'^3}{K\sqrt{K}}-\frac{\frac{1}{2}H'K''}{\sqrt{K}}+\frac{\frac{1}{4}H'K'^2}{K\sqrt{K}}=0\,;$$

viz., this is $$2(H''K'-H'K'')K+(K'+H')K'^2=0,$$

which is satisfied by $K'+H'=0$ or $K+H=\beta$, β an arbitrary constant. Substituting for K, H their values, this is

$$(\phi+\alpha)^2+\sin^2\theta=\beta,$$

that is, $\alpha^2-\beta+(\phi^2+\operatorname{cosec}^2\theta)+2\alpha\phi=0$, or, what is the same thing, $A+B(\phi^2+\operatorname{cosec}^2\theta)+C\phi=0$, where the ratios $A:B:C$ are arbitrary.

III. "The Proteids of Serum." By W. D. Halliburton, M.B., B.Sc., Sharpey Physiological Scholar. Communicated by Professor Schäfer, F.R.S. (From the Physiological Laboratory, University College, London.) Received May 28, 1884.

(Preliminary Notice.)

The investigation of which this paper gives a brief summary relates to serum-albumin rather than to serum-globulin, and the experiments may be arranged in two categories: first, those relating to heat-coagulation; and secondly, those relating to the action of certain salts upon the proteids of blood-serum.

The apparatus used for the determination of the temperature of the heat-coagulation of proteids was not that which has been usually employed for the purpose, and which consists of two beakers containing water, one within the other, and heated gradually over a sand-

bath; the substance under investigation being placed in a test-tube contained within the inner beaker.* The chief objection to that method is that the rise of temperature in the water in the beakers takes place with extreme slowness, so that changes are apt to occur in the proteid during the experiment. To meet this difficulty an apparatus was devised by Professor Schäfer, which was found to be extremely easy to use, and of which the great advantage consists in the readiness with which a constant temperature is maintained for a considerable time. It may be briefly described thus: the liquid of which one wishes to determine the temperature of coagulation is placed in a test-tube in sufficient quantity to cover the bulb of a thermometer put into it; the test-tube is placed in the neck of a flask containing water; this water is kept at the desired temperature by the following means. It is in the first place kept constantly running, entering by one tube and leaving the flask by another tube inserted as a T-piece in the upper part of the neck. The water is warmed by passing it through a coil of tubing contained in a vessel in which water is kept constantly boiling. By regulating the rate at which the water flows through this apparatus the desired temperature is maintained.

The temperature of coagulation of serum-globulin is stated to be 75° C., and that of serum-albumin as 73° C. But it was found that after heating serum to the higher of these two temperatures, and filtering off the coagulum, that the filtrate still contained a large amount of proteid, which coagulated at a temperature higher than 75° C.

A method of fractional heat-coagulation was therefore adopted. A specimen of serum was taken, and heated until a flocculent precipitate appeared; this was filtered off, and the filtrate again heated until another deposit took place; this was repeated until the filtrate gave no evidence of the presence of proteid. In order to obtain these precipitates it was necessary to render the liquid faintly acid; if this is not done alkali-albumin is formed, which will not coagulate. It is also necessary to render faintly acid each filtrate, as after the separation from it of a precipitate, a faintly acid liquid becomes neutral, or faintly alkaline. Acetic acid was the acid used for this purpose in most instances.

By this means it was found that the albumin of serum can be differentiated into three different proteids, which coagulate at the temperatures of 73° C., 77° C., and 84° C. These temperatures vary within certain limits, the presence of excess of certain salts to be afterwards noted raising or lowering the temperature somewhat; and it is found that the amount of acid present has a very marked effect

* Gamgee's "Physiological Chemistry," p. 15.

in altering the temperature up to a certain limit, the greater the excess of acid present the lower being the temperature of coagulation. A series of experiments to define precisely what this relation is is at present in progress. The serum of several animals has been examined, viz., the ox, sheep, horse, dog, rabbit, pig, cat, monkey, man. The plasma of two of these animals, viz., the dog and monkey, were also examined, and gave confirmatory results.

The experiments relating to the plasma and serum of the dog will now be detailed at greater length, as an illustration of the method used.

1. A portion of plasma, prevented from coagulating by the addition of sodium sulphate, was rendered faintly acid, and at the following points coagulation occurred:—

56° C. White flocculent precipitate; filtered off; filtrate faintly alkaline; it was rendered faintly acid, and heated to—

73° C., when a dense flocculent precipitate occurred. This was filtered off; the filtrate, again alkaline, was rendered acid, and heated to—

75° C., when a small amount of a finely divided precipitate occurred; from the filtrate, after this always rendered faintly acid, coagula separated at—

77° C. and—

82° C. After filtering off this last precipitate, the filtrate was found to be free from proteid.

2. A portion of plasma was saturated with powdered magnesium sulphate; a precipitate was so obtained which Hammarsten has shown to consist of globulin. This was filtered off, and the filtrate then treated as the original plasma; at the points 73° C., 77° C., and 82° C., white flocculent precipitates were found to occur. The precipitate caused by magnesium sulphate was washed with saturated solution of magnesium sulphate, and then dissolved by adding distilled water. An opalescent solution was so obtained, and by the process of fractional heat-coagulation precipitates were found to occur at 56° and 75°; the filtrate after the latter containing no proteid.

3. After heating another portion of plasma to 75° C., and filtering off the coagulum, magnesium sulphate produced no precipitate, showing that all the globulins went down at or below 75° C.

4. With the serum of the same animal, the dog, treated in a similar manner, the following are the points at which coagula appeared: 70°, 75°, 77°, and 82—83°.

5. With another portion of serum, to which saturated solution of sodium sulphate had been added, in the same proportions in which it had been added to the plasma to prevent coagulation, the points at which coagula fell were 73°, 75°, 77°, and 83°.

6. Another portion was saturated with magnesium sulphate, and the resulting precipitate filtered off; the filtrate gave precipitates at 73°, 77°, and 83°. The precipitate produced by magnesium sulphate after being washed and dissolved was found to coagulate entirely at 75° C.

Thus it is the albumin of serum and not the globulin which is thus differentiated by heat-coagulation into three several proteids with different temperatures of heat-coagulation.

Results similar to these, the points varying a degree or two in various cases, were obtained from the serum of the cat, monkey, rabbit, pig, and man. In most instances the blood of several of each of these animals was examined. In one specimen of monkey's serum no coagulum occurred after 77°; the total amount of proteids in this specimen was, however, very small. In one or two instances a fourth coagulum at 87—88° was found to occur, but this was quite exceptional.

In addition to examining the blood-serum of man, the various so-called serous effusions have been submitted to similar analysis. For these I am indebted to Dr. Henry Maudsley, Resident Medical Officer, University College Hospital, and to others of the resident staff there. It may be briefly stated that all gave similar results, viz., that by a process of fractional heat-coagulation the albumin of serum can be differentiated into three separate proteids. The fluids examined have been hydrocele fluid, pleuritic fluid, ascitic fluid, and parovarian fluid.

Is alkali-albumin a constant constituent of the blood-serum? In one or two instances neutralisation produced a faint cloudiness, but as a rule it did not. After dilution, neutralisation of the serum was found invariably to produce a precipitate; excess of acid up to a certain point increased this, then dissolved it; on collecting this precipitate and examining it, it was found to be serum-globulin.

On making some alkali-albumin artificially from egg-albumin, it was found that magnesium sulphate precipitates it completely, in the same way that that salt precipitates serum-globulin. If alkali-albumin is present in serum, it will therefore be precipitated with the globulin, when the serum is shaken with powdered magnesium sulphate. This precipitate, however, was found when redissolved to coagulate entirely at 75°, and filtering off the coagulum the filtrate showed no evidence of proteid.

The blood of the sheep, ox, and horse was found to differ from those just described, as in them the serum-albumin can only be differentiated into two proteids by the process of fractional heat-coagulation; the points at which precipitation occurs being 77° C. and 84° C.

The albumins of serum having thus been shown to be more numer-

ous than has hitherto been supposed, my next object was to find some distinction between these varieties of serum-albumin other than that of heat-coagulation. It must, however, be remembered that the mere difference of temperature of coagulation is an important one; in fact, one of the chief differences between fibrinogen and serum-globulin is that the former coagulates at 56° C. and the latter at 75° C., and yet the function of these two bodies is vastly different. In the want of knowledge of the chemical constitution of the proteids, one has to be content at present with mere physical properties.

My object, however, was to find certain salts which would precipitate these varieties of serum-albumin. In the same way that Hammarsten discovered that magnesium sulphate will precipitate serum-globulin from serum, and will precipitate nothing else, so it was hoped that certain other salts would precipitate one variety, and that one only, of serum-albumin.

On this point I am at present still engaged. A large number of salts have been examined, and these experiments constitute the second category spoken of at the commencement of this paper.

The most important result obtained has been with sodium sulphate.

When sodium sulphate is added to saturation and in excess to serum, and shaken for many hours, no precipitate occurs.

When magnesium sulphate is used a dense precipitate occurs; this is filtered off, and then the filtrate is saturated with sodium sulphate; another dense precipitate occurs.* If the filtrate saturated with magnesium sulphate be shaken with sodium sulphate in excess for eight or nine hours, the serum-albumin is found to be completely precipitated; and the filtrate is found to be absolutely free from proteid, and is clear and colourless.†

If this precipitate be washed with water saturated with magnesium sulphate and sodium sulphate, and then dissolved by adding water, the resulting solution will be found to contain proteid, which can again be separated by heat-coagulation in the way previously stated.

This experiment has been performed with sheep's serum, and has been confirmed on rabbit's serum.

Potassium acetate was found to act similarly to sodium sulphate; adding it to saturation to serum already saturated with magnesium sulphate produced complete precipitation of all forms of serum-albumin. Potassium acetate differs from sodium sulphate in also producing a precipitate in simple serum. Here it precipitates both globulin and serum-albumin. It thus resembles in its action potassium carbonate, which is known to precipitate all the proteids. Potassium acetate is a difficult salt to work with, as it deliquesces in

* Schäfer, "Journal of Physiology," vol. iii, pp. 181–187.

† I have since ascertained that if serum is saturated with the double sulphate of sodium and magnesium, all the proteids are quickly and completely precipitated.

the air, and is so soluble that a vast amount of it is required to saturate even a small quantity of serum.

Various other salts have been examined. Many that are not very soluble produce no effect at all; among these may be mentioned potassium nitrate, potassium sulphate, potassium chloride, potassium chlorate, ammonium chloride.

Others, like sodium chloride and sodium nitrate, precipitate globulin completely, but slowly, in simple serum. Sodium nitrate precipitates serum-albumin completely, but slowly, after saturation with magnesium sulphate.

Sodium nitrate has a very marked effect in lowering the heat-coagulation of a proteid. Potassium nitrate acts similarly, but not nearly to so great an extent as sodium nitrate.

Fuller particulars concerning these various salts will be given when the experiments relating to them are completed.

The most important results that these researches show are as follows:—

(1.) That the albumin of serum can by the process of fractional heat-coagulation be shown to consist of three separate proteids, which may be called provisionally serum-albumin numbers 1, 2, and 3 (to avoid a multiplicity of terms). In certain animals only two of these are present.

(2.) That the precipitates described by Fredericq as occurring in serum at temperatures below 70° C. have, in this series of experiments, in which some hundreds of heat-coagulation determinations have been made, been found never to occur.

(3.) That the albumins of serum can be completely precipitated after saturation with magnesium sulphate and removal of the serum-globulin by saturation with sodium sulphate.

(4.) That potassium acetate, added in excess to solution of a proteid, *e.g.*, serum or solution of egg-albumin, completely precipitates the proteid therefrom without coagulating it.

IV. "The Influence of Stress and Strain on the Physical Properties of Matter."* Part I. Moduli of Elasticity—*continued*. Relations between Moduli of Elasticity, Thermal Capacity, and other Physical Constants. By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received May 28, 1884.

(Abstract.)

The thermal capacity of each of the wires already used for the

* The original title of the paper has been altered to the above, as being more exact in expression.

experiments on moduli of elasticity and electrical conductivity described in Parts I and II of this paper* was determined in the following manner:—The wires, which had been previously well annealed, were wound round a steel rod and so made into coils of length about 2 inches, inner diameter $\frac{3}{4}$ inch, and outer diameter $1\frac{3}{4}$ inches, the rod was then withdrawn and the coil inserted into a thin brass envelope. The envelope consisted of a hollow cylinder 2 inches in length and 2 inches in diameter, terminated at both ends by a truncated cone. One of the conical ends was closed and could be unscrewed so as to receive the coil of wire, and was, after the insertion of the coil, screwed on again. At the other extremity of the envelope was an aperture $\frac{3}{4}$ inch in diameter, through which a thermometer could be inserted, so that its bulb would lie along the axis and in the centre of the coil of wire. The brass envelope and the contained coil were heated in an air-chamber consisting of two concentric copper cylinders of equal length connected at their two ends, so as to enclose between them a layer of water $\frac{1}{4}$ inch thick. The length of the air-chamber thus formed was 14 inches, and the inner diameter $2\frac{1}{4}$ inches, so that the envelope could freely slide through the chamber. The two ends of the air-chamber, which was placed in a horizontal position, were closed by corks, through the centre of one of which passed a thermometer, and through a very small hole in the centre of the other was drawn a fine but strong thread attached with one extremity to the envelope, and with the other wound round a little piece of wood which prevented the thread from being dragged through the hole when the former was drawn tight. The envelope and contained wire were heated before immersion in the water of the calorimeter in one set of experiments to about 60° C., and in another set to about 100° C. The envelope served a double purpose, as it not only enabled compensation to be made for loss of heat during the transference of the wire from the air-chamber to the calorimeter, but also was of use in distributing the heat uniformly throughout the coil.

Preliminary observations were made for the purpose of ascertaining the rise of temperature which would be caused by immersing the heated envelope only in the water of the calorimeter.

Every precaution was taken both with regard to the instruments themselves and the mode of using them to avoid error, and the formulæ given below may be received with great confidence.

It will be seen that the thermal capacity of all the metals examined increased with the temperature, a result which we find confirmed by the observations of other investigators.

The thermal capacities of the alloys platinum-silver, and German-silver are, within the limits of error, exactly the same as those

* "Phil. Trans.," Part I, 1883, p. 1.

Metal.	Density at 20° C., density of water at 4° C. = 1.	Formulæ for the number of thermal units required to raise the temperature of unit mass from 0° C. to $t°$ C. Thermal capacity of water at 0° C. = 1.	Thermal capacity per unit mass at $t°$ C.
Aluminium	2·731	$\cdot 20700t + \cdot 0001152t^2$	$\cdot 20700 + \cdot 0002304t$
Iron	7·759	$\cdot 10601t + \cdot 0000701t^2$	$\cdot 10601 + \cdot 0001402t$
German silver	8·632	$\cdot 09411t + \cdot 0000053t^2$	$\cdot 09411 + \cdot 0000106t$
Zinc	7·138	$\cdot 09009t + \cdot 0000374t^2$	$\cdot 09009 + \cdot 0000748t$
Copper	8·851	$\cdot 09008t + \cdot 0000324t^2$	$\cdot 09008 + \cdot 0000648t$
Silver..............	10·464	$\cdot 05466t + \cdot 0000218t^2$	$\cdot 05466 + \cdot 0000436t$
Tin................	7·264	$\cdot 05231t + \cdot 0000361t^2$	$\cdot 05231 + \cdot 0000722t$
Platinum silver	12·616	$\cdot 04726t + \cdot 0000138t^2$	$\cdot 04726 + \cdot 0000276t$
Platinum...........	21·309	$\cdot 03198t + \cdot 0000063t^2$	$\cdot 03198 + \cdot 0000125t$
Lead	11·193	$\cdot 02998t + \cdot 0000153t^2$	$\cdot 02998 + \cdot 0000306t$

calculated from the proportions of their components. Thermal capacity is, therefore, a physical property which is not likely to be altered to any appreciable extent by small impurities, so that the results obtained by different experimenters agree very closely with each other.

It has been proved* that if e be taken to denote "Young's Modulus," and α the mean distance between the centres of two adjacent molecules, $e \times \alpha^7$ is in the case of most metals approximately a constant. From this it would follow that the law of force proved by Maxwell in his experiments on the viscosity of gases† to exist between the molecules of a gas is approximately true for solids, accordingly the force between any two adjacent molecules of a solid is approximately as the fifth power of the distance between their centres. Now if we denote the atomic mass by A, the density by Δ, the thermal capacity per unit mass by C_m, and the thermal capacity per unit volume by C_v, we have the following relations :—

$$C_m \times A = \text{a constant};$$

$$C_v = \Delta \times C_m;$$

$$e \times \alpha^7 = \text{a constant};$$

$$\alpha \propto \left(\frac{A}{\Delta}\right)^{\frac{1}{3}}$$

From these relations we obtain—

$$\frac{e}{C_v^{\frac{7}{3}}} = \text{a constant};$$

* *Loc. cit.*, p. 32.

† "Phil. Trans.," 1866, vol. 126, Part I.

or that the cube of "Young's Modulus" varies as the seventh power of the thermal capacity per unit volume. This relation was found to hold approximately not merely for the metals here examined, but also in the case of a great many substances for which the values of C_v and e have been determined by other investigators.

Still more approximately is it believed that this relation would hold good if for "Young's Modulus" the bulk-modulus of elasticity were substituted. Denoting the bulk-modulus by e_v, it was found that, within the wide limits of error to which determinations of the value of the bulk-modulus are liable to be affected

$$\frac{e_v}{C_v^{\frac{7}{3}}}=\text{a constant.}$$

Neither of the above relations can be true for all temperatures, inasmuch as whilst the value of e_v diminishes with rise of temperature, that of C_v increases, but at ordinary temperatures it seems that the bulk-modulus of elasticity in grammes per square centimetre can be calculated from the thermal capacity per unit volume by the formula—

$$e_v=2071\times10^6C_v^{\frac{7}{3}}$$

The thermal capacity per unit volume increases with the temperature, and the researches of Matthiessen, Fizeau, and others on the one hand, and of Kohlrausch on the other, have shown that there is a like increment in the thermal expansibility and torsionability* of metals. A careful comparison was made of the various increments above mentioned, and it is shown in the paper that whilst the ratio of increase per unit of expansibility with rise of temperature to corresponding value in the case of torsionability† is, within the limits of error of observation, unity, that in which thermal expansibility and thermal capacity are concerned is about two, so that the rate at which thermal expansibility increases with the temperature is about twice the rate at which thermal capacity increases. The rate of increase of both thermal expansibility and thermal capacity varies with the nature of the metal, being greatest for iron and least for platinum.

The so-called "real thermal capacity" of a solid may be found by dividing the thermal capacity of hydrogen per unit mass at constant volume, namely, 2·417, by the atomic mass; and this part of the capacity will be independent of the temperature. If the "real capacity" be subtracted from the total thermal capacity we obtain that part of the capacity which does vary with the temperature, and which has therefore in this paper been designated the "variable

* The inverse of "simple rigidity."

† Iron and copper are the only two metals for which the increase of torsionability with rise of temperature has been examined.

thermal capacity." The following table shows that *the rate of increase per unit of thermal expansibility is at 0° C., and therefore at any temperature, equal to the increase per unit of the "variable capacity"* :—

Metal.	Rate of increase per unit at 0° C. of "variable thermal capacity" = C′.	Rate of increase per unit at 0° C. of thermal expansibility = E.	$\frac{E}{C'}$.
Iron	·00230	·00309	1·34
Tin................	·00216	·00250	1·16
Aluminium	·00197	·00215	1·09
Lead	·00192	·00174	0·91
Copper	·00127	·00196	1·54
Zinc	·00157	·00170	1·09
Silver.............	·00135	·00155	1·15
Platinum..........	·00064	·00061	0·95

As a consequence of the above-mentioned relations we can from a knowledge of the rate of increase of the "variable thermal capacity" determine the expansibility *at any temperature* between two limits, provided we know the *mean* coefficient of expansion between those limits. Again, should the rate of increase of torsionability prove for all metals, as it is for iron and copper, equal to the rate of increase of expansibility, we shall, by the method of torsional vibrations, be able to determine with any degree of accuracy we please any one of the three rates of increase.

It is shown in the paper that the thermal capacity per unit mass is nearly two and a half times the "real capacity," so that only two-fifths of the whole thermal energy which we may impart to a mass of metal goes towards raising the temperature, the remaining three-fifths being expended in internal and external work. The external work is practically insensible in ordinary cases. Of the internal work, that expended against bulk-elasticity amounts in the limiting cases from $\frac{1}{1000}$ to $\frac{1}{10000}$ of the whole, and, though greater than the external work, is almost insensible; moreover, there seems to be no relationship whatever between the whole thermal capacity per unit volume and the work done against bulk-elasticity.

Raoul Pictet has concluded* that the amplitude of the oscillation of molecules around their positions of equilibrium may be taken as corresponding to temperature, and in the case of several metals has shown that

$$T \times \beta \times \alpha = \text{a constant},$$

* "Nature," 1879, p. 356.

where T is the melting point temperature reckoned from absolute zero, β the coefficient of linear expansion, and α proportional to the distance between the centres of adjacent molecules. From the above relation combined with those already mentioned we deduce

$$\frac{T \times \beta}{C_v^{\frac{1}{3}}} = \text{a constant};$$

and

$$\frac{T \times \beta}{e_v^{\frac{1}{7}}} = \text{a constant}.$$

The first of these two relations was found to hold good for ten out of twelve metals examined, but for the metals bismuth and antimony the ratio $T\beta : C_v^{\frac{1}{3}}$ is almost exactly *one half* of the ratio obtained for the other metals. It was concluded that for most metals the melting point temperature may be approximately calculated from the formula—

$$T = \cdot 02253 \times \frac{C_v^{\frac{1}{3}}}{\beta}.$$

Where C_v and β represent the mean thermal capacity per unit volume, and coefficient of expansion respectively between 0° C. and 100° C.

The second of the two relations was found also to approximately hold good.

Altogether the following relations have been approximately established by experiment:—

(1) $$\frac{e^{\frac{1}{7}}}{C_v^{\frac{1}{3}}} = \text{a constant},$$

or more probably $$\frac{e_v^{\frac{1}{7}}}{C_v^{\frac{1}{3}}} = \text{a constant};$$

(2) $$\frac{T\beta}{C_v^{\frac{1}{3}}} = \text{a constant},$$

(3) $$\frac{T\beta}{e^{\frac{1}{7}}} = \text{a constant},$$

or more probably $$\frac{T\beta}{e_v^{\frac{1}{7}}} = \text{a constant}.$$

In these relations we may substitute $\frac{\Delta}{A}$ for C_v, where Δ is the density and A the atomic mass.

In the paper will be found a full discussion of the experiments of Joule* and Edlund† on the thermal effects produced by mechanical stress in metals.

* "Phil. Trans.," 1859, vol. 149, p. 91.

† "Ann. de Phys. und Chemie," Band cxxvi, p. 539.

According to the researches of the latter the *observed* thermal effects of longitudinal stress on a wire is to be found by dividing the *theoretical* thermal effects by 1·61, since part of the work expended on a wire which is stressed longitudinally finds its equivalent in molecular effects which are not thermal. This view seems to be partly supported by some experiments made by the author on the viscosity of metals.

V. "Note on Boiling in a Vessel contained in a Water Bath." By CHARLES TOMLINSON, F.R.S. Received May 31, 1884.

In the "Phil. Trans." for 1673, No. 97, among the "Acta Medica" of Dr. Bartholin, the twelfth is thus stated:—

"A contrivance of making water not boyl in the midst of boyling water, by hanging a narrow-mouth'd glass, half-full of water, in the midst of an Iron Kettle filled with water; whereupon the ambient water may by a strong fire be made to boyl, when as the water in the glass, though it be hot, yet will not boyl at all, though some few bubbles be seen at the bottom, which do all vanish before they come to the top."

In "Rozier's Journal" for 1773, p. 1, is a memoir entitled "Expériences et Phénomènes singuliers sur la Communication de la Chaleur, par M. Braun, de l'Académie de St. Pétersbourg." In this memoir, reference is made to a paper by Olaus Borrichius in the "Memoirs of the Academy of Copenhagen," entitled "Aqua in medio aquæ non ebulliens."

In M. Braun's experiments, a copper vessel was filled with water, and another copper vessel, containing water to the height of two-thirds, was placed in it, so that the level of the water in the outer vessel was above that in the inner. The water in the outer vessel was made to boil violently during upwards of an hour, and the water in the small vessel did not show the least sign of ebullition. It remained in fact 9° (De Lisle's ther.) below the temperature of the water in the outer vessel.

The experiment was also tried in vessels of glass, earthenware, iron, &c., with the same result. Also with different liquids, such as spirits of wine of various densities, contained in both vessels, when the temperature of the liquid in the smaller vessel was from 4° to 12° or 13° below that in the outer vessel. With various kinds of wine, the difference was 4° or 5°; with milk 7°; and with petroleum from 15° to 20°. These results, the author remarks, form a strange paradox, but may possibly be referred to the fact that the outer vessel is immediately in contact with the source of heat.

In "Watson's Chemical Essays" (vol. i, 5th Edn., 1789, p. 73, note) the experiment is again described in the following terms:—

"It is a very remarkable phenomenon that a vessel containing water will never *boil*, how long soever it be exposed to the action of *boiling* water. A common bottle is filled with water, and placed in a pan of water so that the mouth of the bottle be a little above the water in the pan. The pan is set on the fire, and its contents made to boil violently at 212° F., while the water in the bottle will only reach 202°."

In repeating this experiment, I used for the outer vessel a large wide-mouthed flask filled about two-thirds with water, heated by means of a spirit lamp; and for the inner vessel a glass tube, suspended by means of a string attached to the top ring of the retort stand which supported the flask. In this way the tube could be raised or lowered so as to place the level of the water contained in it above or below the level of the water in the flask.

The experiment, under this form, has been frequently repeated, and it was found that, in general, after the water in the flask had been boiling for some time, the temperature of the water in the tube was not more than $1\frac{1}{2}$° or 2° F. below that in the outer vessel.

The inferior temperature of the water in the tube is evidently due to evaporation; and as there were no very clear ideas on this subject until the time of Dalton, the early observers regarded the result as paradoxical.

On covering the water in the tube with a layer of oil, the temperature rose to that of the water in the flask, and bubbles of steam escaped freely from the surface of the water through the oil.

The oil used was olive in one case, and a volatile oil of high boiling point, such as cajuput, in another. On passing the thermometer through the oil down into the tube, bubbles of steam were given off freely from the bulb and stem, thus further illustrating one of the points maintained in my paper on the action of nuclei in liberating vapour from boiling liquids, contained in the "Proceedings of the Royal Society," vol. 17, p. 240.

VI. "Notes on the Microscopic Structure of some Rocks from the Andes of Ecuador, collected by E. Whymper. No. III. Cotopaxi and Chimborazo." By Professor T. G. Bonney, D.Sc., F.R.S. Received June 6, 1884.

In regard to these two important volcanic mountains of the Ecuadorian Andes, Mr. Whymper has favoured me with some descriptive notes, which appear to me of so much interest that I incorporate them with but slight modification into my paper.

Cotopaxi.

"Two volcanoes in Ecuador—Sangai and Cotopaxi—are always in a state of activity, and, of these two, the latter is the higher and the more important mountain. Cotopaxi, according to my observations with mercurial barometer on its summit, is 19,650 feet high, and it is the second in altitude of the Andes of Ecuador, being inferior only to Chimborazo.

"It is situated approximately north-east of Chimborazo at a distance of 65 miles, and lies south-east of Quito at a distance of about 32 miles. It is nearly due east of the Tiupullo ridge (11,550) over which the road to Quito passes, and few mountains look so imposing as it does from this direction—the atmosphere of smoke and haze which is always hanging about it, subduing its details without concealing its general contour, produces an effect of stupendous size and enormous height.

"The volcano proper, as regards its north, west, and southern sides, may be considered to rise from a plateau, which is elevated about 11,500 feet above the sea. I have only seen the eastern side from considerable distances, and, as well as I can judge, the mountain would appear to reach a lower level on the east than upon any other side. No houses are, so far as I am aware, situated within 10 miles of the crater—the nearest, on the west, being at Santa Ana (10,335) on the Quito road; on the south-west by west at the village of Mulalo (10,036); and on the north-north-west at the hamlet of Pedregal (11,628).

"To judge correctly of the general form of this volcano, it is necessary to view it at a considerable distance. At 15 miles, or further away, the upper 8,000 or 9,000 feet present the appearance of a very regular and slightly blunted cone. As the longer axis of the crater has a northerly and southerly direction, the mountain looks sharper when viewed from the north and south than it does when seen from the west. The symmetrical appearance of the volcano diminishes considerably upon a near approach, as its slopes are extremely rugged, and its subordinate features acquire undue importance through the effect of foreshortening. Although much snow falls upon the upper 4,000 or 5,000 feet of the mountain it seldom wears a snowy aspect, through the ash which is continually being ejected quickly blackening it, and from rapid liquefaction, owing to the warmth of the cone.

"With the view of inspecting the interior of the crater (respecting which apparently contradictory accounts had been given by those who had previously ascended the mountain), I proposed to encamp close to the summit, in order that we might see the crater by night. The first day's march took us from the town of Machachi (9,850) to the hamlet of Pedregal (11,628), and the second day to the lower

slopes of the mountain, where we encamped, on its west-north-west side, at the height of 15,100 feet. Bad weather for two days prevented the establishment of a camp upon the summit, but on February 18, 1880, a tent was placed on the western side of the terminal cone, on the outside, about 130 feet below the highest point. The slope was entirely composed of ash, piled up almost to the maximum angle at which it could repose, and it was necessary to cut deeply into it to obtain a sufficiently secure platform. A maximum thermometer placed on the floor of the tent rose to 110° F., and water boiled at 179°. The exterior temperature at the same time ranged from 15° to 20° F.

"Our first (and lower) camp was made on the side of one of the numerous lava* streams which are found on the west of the mountain, and the tent was pitched upon beds of ash, which extended considerably lower down than our position. The volcano, although in a state of activity, was not in eruption. Explosions or violent noises were frequently heard in the bowels of the mountain, resembling such sounds as may be produced by slamming doors at the ends of long passages in a large building. They caused scarcely any tremour. On the way up, when about 1,500 feet below the summit, we were incommoded by puffs of strongly sulphurous vapour, proceeding apparently from fissures in the cone; but when encamped upon the summit, during a stay of twenty-six hours, we neither heard such noises as those referred to, nor were we in any way inconvenienced by the vapours proceeding from the crater.

"Steam and smoke were continually issuing from the crater and simmering over the edge, but we heard no extraordinarily loud noises, and for some hours devoted our whole attention to firmly establishing the tent, which was a matter of no little difficulty, owing to the unstable nature of our platform and the high wind which prevailed. The mountain, according to the general testimony of the natives in the neighbourhood, had been unusually quiet for some time past. As this calm might be interrupted at any moment, I directed my people that, in the event of an eruption, everything was to be abandoned, and each was to shift for himself as best he could.

"When our tent was well secured, we went up to the edge of the rim to inspect the interior. Little could be seen of it, even of the part immediately underneath us. The rock of the rim was a compact and very tough and heavy trachyte (a specimen is described below), and the immediate summit of the rim was everywhere jagged and irregular. Some points upon it rose 100 to 150 feet above the general level. At no time during daylight on the 18th could we see all round the rim at any one moment, but we made the circuit of about one-

* This stream is called Yanasache.

third of the crater, and gradually inspected the whole of it as the wind shifted the smoke hither and thither.

"On the first occasion that we mounted to the edge of the rim, we had been standing there only a few minutes, when a loud roar occurred at the bottom of the crater, and volumes of steam were ejected, which rose to our level in two or three seconds, spread out in vast clouds, and were gradually dissipated. We looked at each other with alarm, not knowing what was going to happen. We probably all thought alike—that an eruption was about to occur, and that we had best run for our lives. As quiet reigned again, we remained, and found that such explosions occurred about every half-hour during our stay on the summit. The vapour ejected appeared to be pure steam, and it appeared to issue from the (roughly) circular orifice at the bottom of the crater. It rose with great rapidity, taking, I think, not more than two to three seconds to rise the 1,200 feet from the bottom of the crater to the level of the rim. When it reached us it could not be said even to be warm, and it was quite devoid of odour. The explosions were momentary—the affair of an instant—and during the instants of their occurrence the noise resembled that which is made when large ocean steamers are blowing off steam.

"In course of time, as eddies of wind shifted the vapour in the crater from one to another part, we gradually made out its general form and nature. Its form above is roughly oval, and at the bottom rudely circular. The general angle from the edge of the rim to the bottom exceeds 45°. The mean of a great number of observations would perhaps be about 48°. At no part is there a continuous slope from top to bottom. The amphitheatre is made up of a large number of cliffs (often vertical, and even overhanging), and of slopes of all degrees. This character continues all the way down. The upper part was not noticeably fissured, but from about half-way down, down to the bottom, cracks and fissures were numerous; and from these apertures vapours of various tints were lazily issuing—some white, greyish, or dark, but none were inky black. These vapours perpetually obscured the inferior part of the crater, the influence of the wind not being felt so low down. From this cause, I am unable to state positively from what part the ejections of steam occurred which have been referred to above.

"In the evening of February 18th, when it became dark, I went again to the edge of the crater to view the interior. Less smoke appeared to be issuing from the cracks at the bottom, and it did not now prevent the examination of any part. The fissures for several hundreds of feet from the extreme bottom of the crater were now radiant with heat, and the lowest part was occupied by a circular, glowing, fiery spot, which I estimated to be 180 feet across, or about one-tenth of the entire diameter of the crater. I examined

this a long time attentively with an excellent field-glass, without being able to determine whether this spot represented the upper end of a pipe filled with molten lava, or whether it was only incandescent matter. Flames were flickering and travelling about it in all directions. I compare the appearance they presented to that seen when a flat dish is half covered with lighted spirit. The flames that I saw travelled to and fro in the same peculiar manner. Besides the flames which were at the surface of the orifice or pipe of the crater, there were others in many places flickering over the glowing fissures. The appearance and behaviour of these flames gave me the impression that there was a comparatively tranquil atmosphere at the bottom; anyhow there was nothing that suggested the existence of strong blasts issuing from the fissures.

"After spending a considerable time in the examination of this remarkable spectacle we returned to the tent, and divided the night into three watches. At 11 P.M., when I was asleep, an explosion occurred which caused the lantern suspended from the ridge of the tent to oscillate to and fro—at least so I was informed by my assistant, J. A. Carrel, who was keeping watch at the time. During my own watch nothing remarkable occurred. The ejections of steam could be heard going off from time to time; but the noise made on these occasions (as heard in the tent placed on the *outside* of the crater) was not alarming. The steam sometimes boiled over the edge of the rim, and enveloped the tent. Temperature fell during the night to 13° F.

"We were astir at daybreak on the 19th, and went again to the edge of the rim. The bottom of the crater was again indistinguishable by reason of steam, but the upper part of it was reasonably clear. I photographed a part of it, and measured directly 600 feet along the rim by means of a fine line, and, by taking angles with a theodolite from the two ends of this base, found that the diameter of the rim was about 2,000 feet from north to south, and 1,500 feet from east to west.

"Our tent was rendered a dusky grey colour by fragments of scoriaceous matter, which were ejected during our stay. A quantity of this was collected (described below). These minute fragments were evidently thrown out by the steam blasts, though it should be said that I did not on any single occasion notice anything ejected with or falling from the steam.

"After 8 A.M. on the 19th the wind became dangerously high, and we could not remain upon the rim, but we stayed near the summit until 2 P.M., re-ascending from time to time when the weather moderated. We arrived again at the first camp on the evening of the 19th, and at Machachi on February 24th.

"The following supplementary remarks occur to me:—

"Although the final cone of Cotopaxi is externally principally coated with a deep bed of ash, and is, upon the whole, very regular in form and slope, and is almost featureless, it may be remarked: (*a.*) That on its northern side, extending close up to the rim of the crater, there is a large piece of almost perpendicular precipice, upon which neither ash nor snow can repose. This resembles the great cliffs which I have referred to as occurring near the summit of Chimborazo on its southern and north-western sides. (*b.*) That although the ash upon the final cone of Cotopaxi is exceedingly warm (at a depth of 8 feet in it we found it had a temperature of 110° F.), there are intermixed with it streaks of snow and ice, which, being blackened, are not readily perceived, and are somewhat dangerous to the traveller.

"From a little below the foot of the final cone (say 1,500 feet below the summit) I observed glaciers on each side of the ridge we ascended, which glaciers extended downwards for 1,800 feet, and perhaps considerably lower. I could neither trace their beginnings nor their ends, owing to the large amount of ash and *débris* with which they were coated.

"At no part of the country which we traversed on the south-west and west of Cotopaxi was the soil much fissured by earthquake cracks; but the region on the south, and bearing round to the south-east, was cracked and fissured in the most extraordinary manner. We had a good distant view of this from our first camp, and I much regretted that the constant demands on my time rendered it impossible for me to examine it more closely.

"The country which we traversed on the west of Cotopaxi, at a distance of 5 to 10 miles from the crater, was covered with very numerous rudely spherical masses of scoria, which had been ejected apparently by the volcano; and the same were noticed near the Quito road, south-south-east of the crater, at even greater distances. These masses measured 3 or 4 feet diameter downwards. I could not find any person who had ever known a considerable fragment to be ejected, and the natives in general seemed inclined to ridicule the idea that they had been thrown out." (E. W.)

The specimen taken from the summit ring of the crater of Cotopaxi is a compact rather heavy rock (specific gravity=2·656), of a dull warm-grey colour, faintly speckled or mottled with white spots. It has a roughish subconchoidal fracture, and on the joint faces is a dull indian-red colour. The specimen is in good condition.

The rock under the microscope is seen to contain a considerable number of crystals of a triclinic felspar scattered about in the groundmass. These commonly vary from about ·02 to ·05 in the longer diameters, but are occasionally both larger and smaller. They exhibit more or less oscillatory twinning, and an occasional zonal structure. The majority appear to be labradorite. The outer angles

are generally fairly sharply defined, but a portion of the crystal often has a fractured outline. There is much variation in the amount of enclosures; in some of the crystals they are almost absent, in some they abound. They are granules and microliths of pyroxene, scales of iron-glance, often apparently formed along lines of fracture, opacite, and brown glass. The outer part of the crystals is generally free from enclosures; the interior is sometimes full of them—the latter case, however, is rare.

The slide also contains a considerable number of grains and crystals of augite and of hypersthene. These seldom exceed about ·01 in diameter, and are often less. Some of the latter mineral are very well characterised by form, dichroism, and extinction. In one part of the slide is a cluster of several augite crystals, with some smaller of felspar, a little iron glance, and a grain or two which I suspect to be olivine (it is colourless, while the other two minerals are tinted, and is more granular in texture, but, unfortunately, does not offer anything wherefrom to measure the extinction angle). There are several granules of iron peroxide scattered throughout the slide. The ground-mass consists of a colourless glassy base, densely crowded with microliths of felspar about ·001 long, probably for the most part oligoclase. The rock accordingly is an hyperstheniferous augite-andesite.

The most interesting specimen from the vicinity of Mr. Whymper's first camp is a compact subvitreous rock, almost black in colour with a few light-coloured specks, having a slightly rough subconchoidal fracture; in short, macroscopically, a very typical augite-andesite of the dark type which so often contains hypersthene.

The microscopic examination fully bears out the above inference. Scattered about in the ground-mass are numerous crystals of plagioclastic felspar similar to those described above, probably labradorite, seldom exceeding ·03 inch in greatest length; also numerous very well-characterised crystals of hypersthene and some of augite, with granules of magnetite. The ground-mass consists of very minute elongated felspar microliths and pyroxenic granules, with much dusty opacite, crowded in a glassy base. This rock also is a typical hyperstheniferous augite-andesite.

According to Mr. Whymper, comparatively little rock *in situ* was met with upon the route which he followed on his ascent of the cone of Cotopaxi, the surface being principally covered by *débris* and ash, and the above are the only two specimens which he collected. But of the more fragmental materials he has brought back several examples.

Three specimens of a dark purple-grey slightly vesicular lava, speckled with small crystals of a glassy felspar, were collected at an elevation of about 12,000 feet. They are rounded in form, and range in size, according to Mr. Whymper, from a diameter of about 4 feet

downwards. Some of these lie in positions to which they could not have been brought by water, and they are doubtless volcanic bombs. They are no doubt augite-andesites; the actual rock having a general likeness to that from the first camp, as well as to the highest rock on Chimborazo (described below).

From an elevation of from 15,000 to 16,000 feet he has brought a number of pieces of a pumiceous scoria of rather glassy aspect. The vesicles are of considerable size, and occupy more than half the volume of each fragment. These specimens are of various shades of dull grey in colour, being rather lighter than the last-named rocks, and without the crystals of felspar. The rock is most probably also an augite-andesite, but is evidently in a more glassy condition than the others, although we must allow something to the greater tenuity of the material.

In fixing the tent at his first camp (about 15,100 feet) Mr. Whymper (as mentioned above) dug through three layers of ash. At the top was a miscellaneous accumulation of fragments, then came a stratum of fine ash, about 7 inches deep, followed by a stratum of slightly coarser material.

The upper of these two strata consists of pumiceous lapilli of a pale grey colour, often from an eighth to a quarter of an inch in diameter, evidently closely allied in lithological character to the lighter scoria, already described, from near this tent. There is with them a certain admixture of finer dust, which chiefly consists of pulverised glass, and has very probably to some extent been formed by attrition since the material was collected. Mineral fragments are occasionally distinguished. On the examination of a small portion mounted on a slide, the mineral fragments appear more common. Most of these are broken crystals, often with tolerably regular edges (defined no doubt by cleavage planes), of a plagioclastic felspar, similar to that described in the rock slides; to several of these minute portions of the scoriaceous rock are still adherent; there are also one or two fragments of a dull green augite, and two (in conjunction) of a brownish-green mineral, which in form, extinction, and dichroism agree with the mineral described in these papers as hypersthene. Probably one or two more fragments occur, but they are less characteristic; the mineral is not common. The lower material is much darker in colour, approaching nearer to the tint of the rock seen *in situ.* There are occasional lapilli, ranging up to the size of a small pea, but the most abundant are granules almost the size of the heads of ordinary pins, or even a little smaller. On closer examination this deposit is seen to consist of a dark scoria, not very vesicular, together with occasional fragments of the usual minerals. These are less recognisable under the microscope than in the case last described, as they appear to be rather smaller and more embedded in a scoria-

ceous crust. The difference, however, is probably of but little importance.

Besides the above specimens, Mr. Whymper placed in my hands seven parcels of volcanic dust. The first (No. I) represents the material strewn upon the roof of his tent, by the explosions which occurred, as described above, during the night which he spent upon the summit. The second (No. II) was obtained under circumstances hardly less remarkable. On July 3 Mr. Whymper made his second ascent of Chimborazo. The morning was fine. Cotopaxi, some 65 miles away, was clearly seen, and at first "was not smoking at all." At 5.40 A.M. an eruption suddenly commenced. "A column of inky black smoke rose with immense rapidity 20,000 feet above the top of the crater, that is to say, to a height of about 40,000 feet above the sea; was then caught by an easterly wind, borne at right angles to its former course, then was taken by a northerly wind and carried down upon us." But at 1.20 the dust had not yet reached the summit of Chimborazo, and did not begin to fall till shortly after the arrival of the party thereon; but during their brief stay (1 hour 10 minutes) it "fell to such an extent as to blacken the plateau all over, so that it lost all resemblance to snow, and looked like a ploughed field. This ash was wonderfully fine, and penetrated everything, filled the working parts of instruments, rendered photography a failure, and almost prevented us from eating, as our mouths became filled with grit directly we opened them . . . . Extraordinary and ghastly effects in the sky." The wind was strong from the north-east, and bitterly cold. The ash continued to fall during the whole of the descent, and during their absence had not only covered but also filled the tent (pitched at a height of 15,950 feet).

No. III is a specimen of the dust of the same eruption, which fell at Ambato, between the two mountains, and about 45 miles from Cotopaxi.

No. IV (from the same) fell at Riobamba, due south of Cotopaxi and at about the same distance as Chimborazo.

No. V is a specimen of volcanic dust collected at Quito (35 miles distant) during a great eruption which occurred in the year 1879, when the material fell in such quantities that there was pitch darkness for a time at midday.

No. VI, another specimen of the same from Chillo, rather more than 20 miles from Cotopàxi, where the flood caused by the eruption did great damage.

No. VII is a sample of an ash collected by Mr. Whymper from hollows and sheltered places in the interior of the crater of Cotopaxi. This he thinks the result of some more violent eruption than usual.

The following is a description of the microscopic structure of these volcanic dusts.

No. I. (Tent.) A grey dust with rather darker specks. The grains range from ·02 inch in diameter downwards, a considerable proportion varying between this and about ·01 inch. They may be thus distinguished:—(A) rock fragments, (B) mineral fragments. (A) These consist of (*a*) chips of colourless or nearly colourless glass, sometimes almost clear, sometimes clouded with ferrite or opacite, and containing microliths of felspar, &c.—chips, in short, of glassy lavas, similar to those described above and elsewhere in this paper; (*b*) rough opaque, or nearly opaque, grains, rather more numerous and larger in size, sometimes translucent at the edges, and including microliths of felspar and augite; these, when viewed with a dark background, have a scoriaceous exterior, and are greyish, blackish, or reddish-brown in colour; they are evidently minute lapilli of an andesitic lava. (B) Among these the following minerals may be recognised:—(*a*) felspar, showing occasionally plagioclastic twinning; (*b*) more rare, augite and perhaps hypersthene. I notice fragments both of glass and of minerals even among the finer dust, together with black specks, probably magnetite.

No. II. (Chimborazo.) Fine dust of a slightly paler and redder colour than the last. The grains which make up this interesting deposit, as indicated by a glance at the slides with the unaided eye, are, as might be expected, decidedly smaller than those which characterise No. I, a very few only attain to a diameter of ·01 inch, and this is barely exceeded. Fragments measuring from ·003 to ·004 inch are common, and they vary from this size to the finest dust; the characteristic of the deposit, so far as I can ascertain, being the presence of grains ranging from about ·001 to ·003 inch. They consist, as before, of rock fragments and mineral fragments. Among the former (A) the rough dark lapilli are rare; the majority being translucent and apparently smooth externally. These are chips of glass, commonly of a pale brownish colour, in which acicular microliths, probably of felspar, are frequent, with specks of ferrite, and possibly a granule or two of a pyroxenic mineral; vacuoles are certainly rare. (B) The mineral fragments are felspar, as above, with a little augite, and there is one well-formed hypersthene crystal ·01 inch long, in which are enclosures of iron peroxide, &c., and, I think, minute cavities. Fragments of felspar and acicular crystallites are rather abundant among the finer dust.

No. III (the dust from Ambato) does not materially differ from No. II, except that perhaps the size of the fragments is rather more variable. Although there is a large quantity of very small chips there is a slightly greater proportion of fragments about ·01 in diameter, and a considerable number of these are scoria, which is almost opaque. In those that are transparent small vesicles, as might be anticipated, are slightly more numerous than in the other cases.

No. IV (the dust from Riobamba) does not appear to me to differ materially from No. II, except that perhaps it is very slightly coarser, and the glass chips appear to be a little more vesicular, and so there is a somewhat larger proportion of fragments of colourless or almost colourless pumice.

No. V (Quito).—A slightly paler dust than No. II, with which its constituents correspond more nearly than with the first, but there are a few more of the opaque scoriaceous fragments described in the latter, and the grains occasionally attain a diameter of ·007 or ·008 inch. In the brown glass chips, however, vacuoles, commonly spherical, sometimes elongated, are abundant; many of them range from about ·0001 to ·0002 inch in diameter, but some are still smaller.

In No. VI (Chillo) there are occasionally fragments about ·01 inch in diameter, but they appear to be less common than in the other slides, and chips not exceeding ·002 inch diameter predominate. These are glass and mineral fragments, as above; lapilli appear to be very rare. I think the glass is rather more vesicular than is the case in No. II, but in all these samples of dusts which have travelled for so considerable a distance from the volcano I do not consider the differences more than varietal, and we have in them the *débris* of a microporphyritic hyperstheniferous augite-andesite.

No. VII.—A dust consisting of dark granules, mixed with light grey and reddish specks. The materials are rather coarse, the granules commonly ranging from about ·01 to ·015 inch. The most abundant are minute lapilli of scoriaceous aspect and dark colour, almost black; in less numbers are glassy whitish and reddish granules: with these occur fragments of felspar, augite, and hypersthene. Of the latter mineral there was a fairly perfect crystal about ·015 long, which exhibited a very marked and characteristic dichroism and extinction. The granular character and comparative coarseness of the dust readily distinguishes it from the other examples, and brings it nearer to those described from the locality of the first tent.

I am indebted to the kindness of Professor Judd, not only for having a series of the above volcanic dusts excellently mounted for me in his laboratory, but also for the gift of two slides of the materials ejected from Krakatoa in the eruptions of last year. Of these, one is a specimen of the pumice found floating in the Sunda Straits, a vesicular clear glass, like some viscid fluid "whipped" to a foam and then allowed to flow, which presents but little resemblance to the materials ejected from Cotopaxi. Neither does the other specimen (from the ash which fell at Batavia, after a journey of 95 miles) present a marked resemblance, for although there is a general similarity in the presence of mineral fragments of the same species, yet the rock fragments differ in the predominance of glasses allied to that

just mentioned; the most common in the Batavia dust being a very light brown "fluted" or possibly porous glass, with occasional chips of the pumice itself. In short, in the Krakatoa rock-materials, "glass foam" predominates, while in those of Cotopaxi we have a much larger proportion of either chips of ordinary glass or more solid scoria. One would, therefore, assume, and this would be in accordance with the far greater violence of these eruptions of Krakatoa, that the molten materials in this volcano had become far more completely saturated with steam at a high pressure before the explosions occurred, which hurled them into the upper regions of the atmosphere. Herr R. O. M. Verbeck, in a very interesting report on Krakatoa, printed in "Nature," vol. xxx, p. 10, states that the steam cloud from Krakatoa on May 20, 1883, must have reached a height of at least 11,000 metres (about 36,000 feet), and during the more violent explosions of August 26 and 27 may very well have reached 15 to 20 kilometres (49,213 to 65,618 feet). Cotopaxi, by its frequent "puffings," exhales, as it were, the imprisoned vapours, and thus its ejectments are less uniformly vesicular. It would be worth noticing whether the materials ejected from volcanoes which had erupted after long intervals of repose were more pumiceous than those discharged from vents where there is always more or less of disturbance.

Chimborazo.

"Chimborazo has for centuries been known to be a very lofty mountain. Humboldt ("Aspects of Nature," vol. i, p. 96) says that for a long time into the present century it was "everywhere regarded as the highest mountain in the world," and it still ranks as the most elevated in Ecuador. From my mercurial barometer observations on the summit on January 4, 1880, and July 3, 1880, its height appeared to be 20,545 and 20,489 feet respectively. The height I adopt, namely, 20,517 feet, is the mean of these observations. According to Humboldt ("Aspects of Nature," vol. i, p. 57), its height is 21,423 feet.

"The summit of Chimborazo bears north-east from Guayaquil, distant about 91 miles. The valley of the River Chimbo forms the boundary of the mountain, properly speaking, on the western side. This river at the bridge of Guaranda is 8,530 feet above the sea. On the north, the depression (used as a pass) between the mountains Carihuairazo and Chimborazo is the northern boundary of the latter. This is 14,400 feet above the sea. On the south it is bounded by the great sandy plain called the Arenal Grande, the highest point of which is about 14,000 feet above the sea, and is traversed by the track to Quito. This plain is covered with *débris* from Chimborazo, which in various places has been re-assorted, and now forms stratified deposits. On the east it is scarcely possible to say where the mountain

terminates. Its lower slopes extend a long distance into the basin of Riobamba, and die out only a few miles to the west of the town of that name."

[From the Arenal Mr. Whymper has sent to me four small parcels of the "sandy" material. The first is a number of fragments of lava, slightly scoriaceous, varying in colour from a pale to a warm grey and speckled with black crystals (probably augite), the largest being about $1\frac{1}{2}$ inch in diameter. Small crystals of felspar appear to be rather abundant, and fragments of this and other minerals are rather frequent in the dust which is mixed up with the coarser parts. Another parcel consists chiefly of rather small lapilli, some being of a pale reddish-brown colour. A third consists of lapilli of the above tint, varying in size from rather larger than a hemp seed to a little less than a mustard seed; and the fourth, like the first, consists of larger fragments mingled with a considerable amount of fine dust, the whole being a little paler in colour than the first specimen. I have not thought it necessary to subject these to a minute examination, as I have no doubt that they have a general agreement in their composition with the rocks of the mountain, and are simply volcanic dust and lapilli, which have been ejected from Chimborazo in the days when it was an active volcano, and are now a good deal decomposed by the action of percolating water.—T. G. B.]

"In plan, Chimborazo is elliptical, and its longer diameter extends approximately from north-east to south-west. In this direction, at the level of 14,000 feet, the mountain extends over about $14\frac{1}{2}$ English miles. When viewed from certain directions Chimborazo is seen to be crowned by two rounded snow-covered points. From true east or west these points conceal each other, and the general appearance of the mountain when viewed at considerable distances from all points of the compass, is that of a cone, the minor details being indistinguishable.

"On a close approach, the subordinate features destroy the cone-like effect of Chimborazo. From the south, the two summits are well seen, but as the traveller bound for Quito passes round the eastern side the mountain gradually takes the appearance of a *range*. From the north-west or north-north-west, at a distance of three or four miles from the mountain, the lofty and almost vertical cliffs which are found on that direction assume great importance, and nearly conceal the snowy domes which crown the true summit.

"The ridges which radiate from the upper part of Chimborazo are too numerous to be specified. Those not covered by snow or glacier are mostly, if not entirely, old flows of lava, which have been greatly eroded, and are much decomposed.* In numerous places they are almost completely buried in their own ruin. The general appearance of the ridges which are enveloped by snow or glacier renders it probable that they too are fundamentally old lava streams.

"The whole of the upper part of Chimborazo is crowned by snow-covered glacier, forming a glacier plateau, from which numerous glaciers take their rise. In passing around the mountain I remarked twelve glaciers, all of considerable size, flowing from this plateau. The glaciers have their greatest development upon the eastern and north-eastern sides of the mountain. There is one to the south-south-west of the second summit, that is wholly formed by the re-consolidation of glacier ice, which falls over the great cliffs at its head. This glacier we called Glacier de Débris. The clean sections which are exposed in the upper glacier by its breaking away and falling over the above-mentioned cliffs show that the ice sheet on the summit is more than 200 feet in thickness.

"I found *roches montonnées* upon Chimborazo, in a valley (or *vallon*) leading in a south-westerly direction from the second summit. In this valley there is not at the present time any glacier, and this is the only place in the whole of the Andes of the Equator where I have observed *roches montonnées* at any considerable distance from existing glaciers. Retrograding and advancing glaciers may, however, be remarked in various places. On the north-eastern side of Chimborazo some very large moraines which are in the vicinity of the existing glaciers, indicate they were of much greater magnitude in some previous period. It may be that many more traces of glacier action would be apparent if the rocks of this region disintegrated less easily.

"The average level to which the glaciers descend on Chimborazo may, I think, be stated at 15,600 feet. The snow line is approximately 1,000 feet higher.

"The whole of the apex of Chimborazo is enveloped by snow-covered glacier, and upon the immediate summit there is a considerable (and slightly concave) plateau, on the rim of which the two crowning snow-domes are situated. The more northern of these two is slightly the loftier, and there can be little doubt that they cover the highest points of the rim of the crater of the old volcano, and that the hollow plateau lying between them is the filled-up crater itself.

"The highest rock which we noticed upon either of our ascents was about 1,000 feet below the summit, a scoriaceous lava, which appeared in small patches, and was apparently in consolidated beds. The highest rock *in situ* beneath this which we were able to collect was at the height of 18,400 feet, from the lower beds of the great cliffs facing the south, which support the second summit. It is a trachyte of a coarse red colour, and is one of the most common rocks on Chimborazo. Above it, disposed in parallel bands, were a large variety of strata of various natures, and of widely different colouring; and upon the rare occasions that these cliffs were lighted by the sun they presented an appearance as attractive as the well-known slopes

of Alum Bay. It was impossible to collect these *in situ*, the cliffs being well-nigh perpendicular; but specimens from all the strata were broken off by the ice falling from the glaciers above, and fell along with the ice on to the Glacier de Débris below. From the surface of this glacier I collected numerous specimens, and they embrace most of the varieties of rock to be found on the mountain. Amongst this *débris* was found a small fragment of native sulphur.

"There is a prominent and important ridge leading from the second summit in a south-south-westerly direction. Our second and third camps (16,600 and 17,300 feet respectively) at which we passed sixteen days and nights, were established on this ridge, which starts from the foot of the above-mentioned cliffs and extends right down to the *arenal grande*. The specimens collected *in situ* from the rocks at the second and third camps are nearly identical in nature.

"Our fourth camp (14,400 feet) was established on the southern side of a very prominent lava stream that starts high up on the mountain on its northern side, which at its commencement descends a little to the east of north, and in its lower course bends round towards the west; and our fifth camp (15,950 feet) was placed about two miles to the south-west of the last named, against some large blocks of lava.

"On the southern side of the mountain there are numerous masses of scoria lying about, in the same manner as is seen around the active volcano Cotopaxi, which may either have been ejected from Chimborazo when it was in a state of activity, or have fallen from its decomposing beds. The whole of the western side of the mountain (which has, I believe, scarcely been visited by earlier travellers) was found between about 13,000 and 15,000 feet to be covered by vast sandy plains.

"It was observed during our sojourns upon and in the neighbourhood of Chimborazo in the months of December, 1879, and January, June, July, 1880, that easterly and north-easterly winds were of most frequent occurrence. These winds usually brought bad weather, and much snow fell while they lasted. To this preponderance of easterly and north-easterly winds I attribute the great development of glacier which is found upon those sides of the mountain, and the occurrence of the vast sandy plains which exist on the western or lee side. The sand is drifted entirely away from the eastern slopes, which, at the corresponding elevation of 13,000 to 15,000 feet, are rugged, fissured, and often troublesome to traverse. The western side, at these elevations, is free from difficulty. All fissures and minor inequalities are completely effaced, and in the month of July, without any guide except the barometer and compass, we made the circuit of the mountain at about the level of 14,000 feet, and struck with certainty the exact spot upon the *arenal grande* at which we desired to arrive.

"Chimborazo appears to have been an extinct volcano for a long period. The great size of its glaciers and complete effacement of its crater; the extent to which its rocks are decomposed and its ridges shattered; and the occurrence of lichens upon almost the highest rocks which were collected, are all indications that it is long since it was in a state of activity." (E. W.)

Mr. Whymper brought back a large suite of specimens from Chimborazo; from these I have selected eight for microscopic examination, the others appearing to me to be either duplicates or decomposed specimens of the same or nearly identical rocks. From the locality of the second camp I have examined two. The first was collected from *débris* which had fallen from a cliff immediately above. Hence though a loose specimen, it represents rock *in situ* at the locality; the rock is a black subvitreous lava with a few light coloured specks, presenting a very close resemblance to that described above from the flank of Cotopaxi; one face of the specimen is scoriaceous. The microscopic character does not materially differ. There are abundant crystals of similar felspar (elongated forms being perhaps commoner) with similar enclosures, also crystals of augite and hypersthene. One or two grains, however, appear to me to be olivine. There is a base of brownish glass, pretty full of microliths (chiefly of felspar), with specks of ferrite and opacite, and perhaps a little pyroxene.

The other specimen is of a type which, as will be seen below, is common on Chimborazo, and presents resemblances to rock already described, especially that from Pamascucho below Nina-urcu on Pichincha ("Proceedings," No. 299, p. 225). This rock is a dullish lavender-grey colour, with crystals of glassy felspars up to about ·1 inch long, and some minute blackish specks, which weather rather a redder colour. Under the microscope the differences from the other are not so great as perhaps might have been expected, the chief one being that the base is a nearly colourless glass. I think it very probable that a little sanidine is present among the felspars. The rock then is only a variety of the hyperstheniferous augite-andesites.

The specimen taken near the third camp of Chimborazo, 17,300 feet, and representing, as described above, the rock which prevails throughout the ridge by which the first ascent was made, from some distance higher than the locality first mentioned, down to the second camp, and even below, is a rock macroscopically related to the one last described, but is a little redder in colour, more vesicular in structure, and with slightly larger crystals of felspar (up to about ⅛th inch diameter). So far as the base and its included microliths are concerned, there is little to add to the preceding description, except that a dusty ferrite is rather abundant, as the colour of the rock would lead us to expect, the larger crystals of felspar do not materially differ from those already described, hypersthene is abundant, undoubted augite

being rare, and there are two or three small crystals of a strongly dichroic hornblende. Also one or two crystals of what appears to be an iron mica. The predominance of hypersthene entitles this to the name of a hypersthene-andesite.

From the fifth camp four specimens have been brought. Of these four are closely related, and are rocks presenting a general similarity to the second specimen of the second camp, and to that from Pamascucho in the Pichincha *massif* as well as one from Guagra-ialina, Antisana, being compact greyish lavas, with small crystals of white felspar. I have examined one microscopically, and find that it then presents some differences. The felspar crystals, indeed, are similar to those already described, except that perhaps they are a little more crowded with microlithic enclosures, but there are no well-defined crystals of augite, hornblende, or hypersthene. Instead of these are rather numerous elongated or rounded bodies, one of the former attaining ·1 inch in length, consisting of an external zone of dusty opacite, containing a less quantity of the same mineral associated with small crystals or specks of a pyroxenic mineral. I have occasionally seen these bodies (for one cannot give them a definite name), and I presume that they are analogous to the cases of replacement of augite by magnetite, which are not uncommon, and are rude pseudomorphs of a mineral of the pyroxenic group, or in some cases possibly of mica.* There are as usual scattered grains of magnetite, and the ground-mass is a glassy base, crowded with lath-like felspar microliths and granules of magnetite. The rock then is an andesite, but of a slightly exceptional character.

The second specimen, Mr. Whymper states, differs from any other rocks which he saw upon the mountain. It is a rather crumbly rock of very irregular fracture, having a very dark grey ground-mass, in which crystals of glassy-white felspar, up to about ·2 inch long, are embedded. When examined microscopically it does not appear to differ very materially from some of those already described, and is very closely related to that forming the ridge of the mountain, different only in the colour of the ground-mass, and, like it, being best named a hypersthene-andesite.

Mr. Whymper brought a large series of rocks collected at an elevation of about 18,400 feet to represent the materials of the cliff on the southern face of the mountain beneath the second summit. They are for the most part andesite lavas, more or less decomposed, varying considerably in tint and in compactness, but evidently closely allied lithologically. Several were not in a condition favourable for examina-

* Specimens in my collection from Auvergne, *e.g.*, from a quarry about three miles above Murat on the high road, from the cliffs of the Puy Cacadogne and the Grande Cascade (Mont Dore) exhibit similar bodies. In some cases the bordered or replaced mineral is hornblende.

tion, and as I feel convinced of their close relationship, I have only made a microscopic examination of two of the best preserved specimens. These are somewhat intermediate between the two specimens mentioned above from Pichincha and Antisana; being compact greyish lavas, with scattered crystals of white felspar. The redder specimen—which most resembles the above rocks, does not, after what has been said, need a detailed description; it is a hypersthene-andesite with a little hornblende. The duller-coloured specimen is an augite-andesite with some hypersthene. The base of each is a clear glass, containing many very minute microliths, probably of felspar, and irregularly clouded with a grey dust and opacite.

Only one specimen remains to be described, the highest rock obtained by Mr. Whymper on Chimborazo, at an elevation of about 19,300 feet. It is a slightly scoriaceous lava, rough to the touch, almost purple-black in colour, with numerous very minute specks of a glassy felspar. Except that the base is rendered rather more opaque by disseminated opacite, it does not differ very materially from several already described. There are the usual crystals of felspar, one or two being much rounded and very full of dull glassy enclosures; there is a fair amount of augite, but no well-characterised hypersthene; so that the rock may be named an augite-andesite.

Thus the rocks of Chimborazo appear to be andesites, and rather closely related; the only variation of any importance being in the amount of hypersthene and the occasional presence of hornblende.

VII. "Notes on the Structure of some Rocks from the Andes of Ecuador, collected by E. Whymper. No. IV. Carihuairazo, Cayambe, and Corazon." By Professor T. G. BONNEY, D.Sc., F.R.S. Received June 19, 1884. Read June 19, 1884.

I have been favoured by Mr. Whymper with some short notes on the structure and physical features of the three volcanic mountains whose rocks are investigated on this occasion, and have prefixed them to my lithological descriptions. It is remarkable what a general uniformity there is in the products of these summits of the Equatorial Andes, and this, as Mr. Whymper informs me, was so obvious that he made but small collections from the mountains which were visited during the latter part of his journey.

Carihuairazo.

"This forms the northern part of the *massif* of Chimborazo. It is separated on its south side from its great neighbour by the depression

called Abraspungo (14,479), and its northern slopes extend almost to the town of Ambato (8,500). The road to Quito winds round its eastern side, and may be considered to mark its boundaries in that direction.

"It is stated by historians that this mountain was formerly loftier than Chimborazo, and that a portion of its apex fell during a great earthquake which occurred at the end of the 17th century. I saw nothing to lead me to suppose that the mountain was at any time much loftier than at present, though it appears beyond dispute that a great fall actually occurred at the above-mentioned period. The part which fell may have formed the northern and eastern side of its crater. At the present time the three peaks which are upon its summit ridge are disposed in a horseshoe form, and I conjecture formed the southern and western sides of a crater which is now buried underneath glaciers.

"We ascended the middle peak of these three, and by mercurial barometer found that its height was 16,514 feet. Messrs. Reiss and Stübel by Δ calculated the height of Carihuairazo to be 16,752 feet. They, however, probably measured the most eastern of the three peaks, which is actually somewhat loftier than the central one.

"The lower slopes of this mountain are very swampy, from which it may be conjectured that there are not so many fissures in the soil as is common in the Ecuadorian Andes, and large thickets of trees, some of considerable age, grow high up its flanks, from which it is reasonable to conclude that it is long since the mountain was an active volcano. The rocks at the time of our expedition were much covered up by snow in the higher regions, and by earth and vegetation on the lower slopes. Such rocks as were exposed appeared closely allied to the specimens collected upon Chimborazo and the other mountains, and we only brought away specimens from the summit of the central peak. These were taken close to its highest point." (E. W.)

The rock appears to be pretty evenly jointed, it weathers brown, and breaks with a rather rough irregular fracture. The colour on this is a warm purplish-grey mottled with darker spots, and speckled with small rather light coloured crystals of felspar with a rather satiny lustre. A few minute vesicles may be perceived under the microscope. The larger felspar crystals in the slide are rather numerous, and commonly vary from about ·05 to ·07 inch in the longer diameter. They exhibit well developed polysynthetic twinning, and are labradorite or a closely allied form. Enclosures of glass or various microliths are occasionally seen, but the majority of the crystals are fairly clear, though a few are very dirty, and have a corroded look at the exterior. There is also present in the ground-mass a fair number of crystals of augite of a yellowish-green colour, not exceeding about ·03 inch in length, and two or three which in structure, dichroism,

and parallel extinction agree with hypersthene. There are scattered crystals of hematite and scales of iron-glance, or possibly small augite crystals iron-stained. The ground-mass appears to be a clear glass thickly studded with dusty ferrite, and with minute crystallites in part, at least, felspar. The rock is, therefore, an augite-andesite, and bears some resemblance to the darker-coloured rock described in Part I from Pichincha.

Cayambe.

"The name Cayambe belongs both to a town and to a mountain. The summit of the latter bears north-east by east from Quito, distant 45 miles. According to my mercurial barometer observations its height is 19,185 feet, and this is remarkably close to the height obtained by Messrs. Reiss and Stubel by $\triangle$, which was 19,161 feet. The French Academicians at the beginning of the 18th century made its elevation slightly greater than my determination, and Humboldt does not appear to have measured it. It is fourth in rank of the Ecuadorian Andes, being inferior in altitude to Chimborazo, Cotopaxi, and Antisana.

"The upper 4,000 feet of Cayambe are almost entirely covered by snow and glaciers, and such small patches of rock as are not covered are indistinguishable at the distance of a few miles. From Quito the mountain has a very noble appearance, but owing to the cloudiness of the atmosphere it is not, perhaps, seen during as much as the fifth of the year. The snow-line on Cayambe is lower than upon Chimborazo, though the former mountain is almost exactly upon the Equator, and the latter is considerably to the south of it. The upper part of Cayambe is a huge hump, and does not appear cone-shaped from most directions. At the level of 9,000 feet the mountain extends about 18 miles from north to south, and 14 to 15 miles from east to west.

"Until the time of my journey, it was conjectured that the mountain was still an active volcano. We inspected the whole of its slopes fairly well all round, and saw no open crater. It is probable that here, as in other of the Ecuadorian Andes, there was a crater near or at the summit which is now filled with snow. It is certain that Cayambe is not now an active volcano, and that it has been one in the past—the numerous streams of lava which are found upon its slopes leaving no possibility of doubt on the subject.

"Very few rocks being exposed upon the upper part of the mountain, I did here as upon Antisana, and collected specimens upon the highest attainable subsidiary peak having rocks uncovered, and from this point (Pointe Jarrin, 16,163 feet), which bears about the same relation to Cayambe as the Aiguille de Gouter does to Mont Blanc, I took specimens of the rocks *in situ*, and of the morainic fragments

lying upon them. There are no rocks exposed at the immediate summit. The highest rock we were able to obtain *in situ* was taken from a small patch of glacier-crowned cliff, some hundreds of feet below the central (and highest) peak on its western side, at a height probably of about 18,800 feet. The culmination of the mountain is an irregular ridge, several hundred yards long, having three distinct snowy bosses." (E. W.)

From the above-named lower summit—the Pointe Jarrin—Mr. Whymper has brought nine specimens, two of which were broken from rock *in situ*. Of the seven specimens collected from the *débris* on the peak, two are rather scoriaceous: one, a small fragment, is a whitish, rather glassy rock, containing small crystals of a glassy felspar, with little plates of black mica and crystals of hornblende (?), in short, a very typical light-coloured "trachyte;" the other is a rather denser rock with a pale reddish matrix and dull whitish felspar crystals, containing apparently less mica or hornblende. The other five are evidently varieties of a rock of the same general character; but one specimen is rather more micaceous than the rest. The remaining four may be described as generally compact rocks, in colour varying from a dull purplish to a reddish tint—the latter being probably due to an alteration in the iron constituent—in fact, I believe the differences in the colour to be mainly the results of weathering. Fairly numerous crystals of whitish felspar, in diameter from about 0·15 inch downwards, but generally not more than 0·1 inch, are scattered in the matrix. I have examined microscopically two specimens from the *débris*, and one of the two from the rock *in situ*. I will describe first the most uniform looking and apparently best preserved specimen from the *débris;* a dull purplish-grey rock, with a fair number of small crystals of whitish felspar, an unequal fracture, and rather clean joint faces. The base is a clear glass, so crowded with specks of opacite and ferrite and microliths of felspar as to have a grey dusty look, except in the thinnest sections. In this are scattered the usual crystals of felspar (plagioclastic, probably labradorite) generally fairly free from inclusions. Magnesian silicates are not very common, but I recognise both hornblende and augite, the former (occurring as the larger crystals) being somewhat black bordered and replaced by opacite, the latter clean and probably belonging to a later epoch in the consolidation of the rock. There are also to be seen scattered grains and crystals of magnetite and perhaps of hematite. The rock has a general resemblance to some of the higher fragments from Chimborazo.

The next specimen (occasionally slightly vesicular, evidently a little decomposed) is of a warm-red colour with crystals of glassy felspar up to 0·2 inch, and a black mica, or hornblende, rather more conspicuous than in the rest. It bears a general resemblance to some of

the "trachytes" from the Euganean Hills. Under the microscope it is seen, like the last described, to have a clear base with microliths of felspar, but scattered in this are numerous rods of iron oxide, and plates of a very ferruginous mica, sometimes all but opaque. There are a few crystals of hornblende in diameter up to about ·04 inch, which are strongly dichroic. There is but little if any augite.

One of the two specimens from the rock *in situ* appeared rather intermediate between the two varieties just described, and as it seemed rather decomposed was not sliced; the other, in better preservation, resembled the more mottled or streaky looking specimens among the *débris*, and afforded slight indications of a fluidal structure. This is indicated under the microscope by a "flow" of the microliths rather than by a marked striping or banding. Except for this, the matrix resembles that of the first described, with some approach to that of the second. There is nothing special to note in the larger felspar crystals which are plagioclastic, similar to those so often described; some are clear, some rather full of glass enclosures. There are fairly numerous hornblende crystals, some of an olive-green, others of a brown-green colour, strongly dichroic. Some augite crystals are certainly present, and a few of a brown mica.

Microscopically the specimen from the highest visible rocks on Cayambe barely differs from that just described, except that a fluidal structure is more inconspicuous. The result of microscopic examination is similar, the differences being but varietal; hornblende, iron-mica, and augite are present, the last being the less conspicuous constituent.

Thus the rocks of Cayambe are very uniform in character, and of the same general type as those of Chimborazo, Antisana (in part), and Pichincha (in part). They are andesites, but as they contain hornblende and augite, as well as mica, it is difficult to give them a distinctive name. I am inclined to view the first, and perhaps the third, as minerals belonging to an earlier stage of consolidation than the second: thus perhaps it is more appropriate to classify these rocks with the augite-andesites, using the word hornblendic as a qualifying epithet, except in the case of the second specimen described above, which might perhaps be termed a mica-andesite.

Corazon.

"The mountain Corazon lies almost exactly midway between Illiniza (17,400) and Atacatzo (14,892), and its summit is nearly due west of the town of Machachi, and south-south-west of Quito. It has received its name from a resemblance which it is supposed to have to a heart. Though a prominent mountain, it is one of the minor ones of Ecuador. Its height, according to my mercury barometer observa-

tions on its summit, is 15,870 feet.* I have frequently seen its entire eastern side quite free from snow, but there are upon its opposite, or western, side some large snow-beds and couloirs which are apparently permanent. The mountain therefore just enters the snow-line.

"The summit ridge is a great wall, about 250 feet long, running (approximately) north and east-south-east, having a sheer precipice on its western, and a very steep cliff on its eastern side. Possibly this wall should be regarded as a dyke. There is no regularly formed crater upon any part of the mountain.

"On the summit ridge I collected specimens of rock *in situ*, and of *débris* lying upon it. The crest of the ridge was almost covered by rock *débris* and earth. Flowering plants were growing up to the very highest point, and vegetation near the summit was more abundant than was found at equal heights on any of the other Ecuadorian Andes."—(E. W.)

From the upper part of Corazon Mr. Whymper has brought eleven specimens. Of these, ten were taken from *débris* scattered about on the summit ridge, and one was broken from the highest rock *in situ*.

The last is a scoriaceous rock with many small cavities and vesicles, darkish grey, slightly inclining to brown in colour. The microscope shows that there is present a fair amount of a glassy base, with a brown staining. This base contains numerous acicular colourless crystallites, and spots—often rather elongated—of opacite, which not seldom are clustered together, and form a kind of frame to the larger felspar crystals. These last are rather abundant, and very commonly do not exceed about ·02 inch diameter, though they are occasionally considerably larger, four or five times the size. The latter often have the dirty look described above. The slide contains a few grains of a pyroxenic mineral, not very distinctly characterised.

The ten specimens gathered at different localities on the summit ridge of Corazon afford the following varieties:—

(A.) A grey pumiceous rock, larger vesicles, a little more than ·1 inch diameter: volume of solid part and hollows about equal: with this may be classed a rather less vesicular specimen, with small felspar crystals; both these have a tendency to weather brown.

(B.) Four specimens of more or less scoriaceous rock of a purplish colour within, weathering externally to an Indian-red colour, and containing specks of white felspar.

(C.) Two specimens of dull grey rock with a rough fracture, speckled with small felspar crystals, and with a spotted look, one being more decomposed than the other.

* By Δ Messrs. Reiss and Stubel made its height 15,801 feet.

(E.) A dark-coloured rock with numerous specks of white felspar, rather glassy-looking outside but with rough internal fracture.

(D.) A rock similar to the last, but in shape a flattish slab rather less than half an inch thick, reminding one of the andesites and phonolites used in Auvergne for roofing purposes.

The specimens (A) were not very well suited for examination, and were clearly only scoriaceous forms of andesites; nearly allied to one of the specimens in (B); of these I have not had slides made.

(B.) The differences in these do not appear to be more than varietal; all are more or less vesicular, but in the one examined the cavities are very small and not numerous; the matrix is a dull purplish-grey, weathering rather red externally, and minute white felspar crystals are abundant. The microscopic structure differs so little from those already described that it will be enough to say that the ground-mass is rather opaque, and that there is present in it the usual plagioclastic felspar, a fair amount of characteristic augite, and a crystal or two of hypersthene.

(C.) The less decomposed of the two specimens has been examined. There are some varietal differences. The larger felspar crystals (labradorite) are not quite so numerous as in the other, while crystallites about ·01 inch or rather less in longer diameter are very numerous. There is a fair amount of well-characterised augite, with grains of iron peroxide and opacite dust in a clear glassy base.

(D) does not very materially differ except that the ground-mass of the slide is rendered more opaque by the presence of opacite.

(E) is a rock of similar character, except that the base is yet more opaque. There is, however, one important distinction, that the greater part of the pyroxenic constituent appears to be hypersthene, and not augite. It is almost impossible to doubt the presence of an orthorhombic mineral in this slide.

On the summit of Corazon Mr. Whymper found two rock specimens, evidently rudely dressed by hand, which will be described in his forthcoming work on the Equatorial Andes. The material of these bore a close resemblance to the rocks described above under the group (B).

From the above remarks it would appear that the crest of Corazon consists of augite-andesites, which only exhibit slight varietal differences, except in the last case (E), where hypersthene becomes rather abundant, apparently predominating over the ordinary pyroxene. It is a remarkable fact that the exterior aspect of the rock had at once reminded me of those black, somewhat resinous-looking rocks—formerly variously called melaphyres, pitchstone-porphyrite, &c.—which, of late years, have been so frequently proved to contain hypersthene.

VIII. "Note of a Theory of Orthoptic and Isoptic Loci." By Charles Taylor, D.D., Master of St. John's College, Cambridge. Communicated by J. W. L. Glaisher, M.A., F.R.S. Received June 10, 1884.

The orthoptic locus of a curve and its isoptic loci are the loci of the points of concourse of pairs of tangents drawn to it at right angles, and at angles equal to given angles, respectively.

As a step towards a general theory of such loci, of which special cases only have been treated hitherto, it is shown below that the order of the orthoptic locus of a curve of class n is $n(n-1)$, and the order of its isoptic loci $2n(n-1)$.

The principle on which our proof depends is that lines drawn from either of the circular points at infinity I and J may be regarded as intersecting at any angle whatsoever,* but such as are drawn from any other point at infinity, real or imaginary, can only be regarded as parallel, unless one of them be the straight line at infinity, which makes an indeterminate angle with any straight line.

1. *The Orthoptic Locus of a Curve of any Class.*

To a curve of the nth class n tangents, constituting $\frac{1}{2}n(n-1)$ quasi-orthogonal pairs, can be drawn from I or J. Each of these is therefore a point of the order $\frac{1}{2}n(n-1)$ on the orthoptic locus, and this locus, having in general no other points at infinity, is of the order $n(n-1)$.

If the curve touches the line IJ in one point, $n-1$ other tangents can in general be drawn to it from any point on IJ, and each of them may be regarded as orthogonal to IJ. Every point at infinity is therefore of the order $n-1$ on the orthoptic locus, and the remainder of the locus when the factor IJ^{n-1} is subtracted is of the order $n(n-1)-(n-1)$, that is to say $(n-1)^2$, and contains I and J as points of the order $\frac{1}{2}n(n-1)-(n-1)$, or $\frac{1}{2}(n-1)(n-2)$.

If the curve touches IJ in r points it appears in like manner that the orthoptic locus contains IJ as a factor $r(n-1)$ times, and the remainder of the locus is therefore of the order $(n-r)(n-1)$, and contains I and J as points of the order $\frac{1}{2}n(n-1)-r(n-1)$, or $\frac{1}{2}(n-2r)(n-1)$.

* To demonstrate the existence of the circular points, draw a circle, and upon it take an arc AB at random, and let x be either of the points in which the circle meets the line at infinity. Any two straight lines through x may be regarded as making zero angles with xA and xB respectively, and therefore as including an angle equal to that standing on the arc AB, which may be of any magnitude whatsoever. It readily follows that all circles pass through x, and hence that there can be only two such points on the line at infinity.

Notice in verification the case of the conics (de la Hire, 1685), and likewise that of the cardioid, whose orthoptic locus consists of a circle and a bicircular quartic, which together make up a tricircular sextic. When the curve resolves itself into n point-factors the orthoptic locus evidently consists of the $\frac{1}{2}n(n-1)$ circles described on the lines joining the points two and two as diameters.

2. *Pedals of a Pair of Curves.*

The locus of the vertex of a right angle whose arms envelope two curves of class m and class n respectively may be called the pedal of the two curves, or of the one with respect to the other, and the corresponding locus generated by the vertex of any other constant angle may be called a skew pedal of the two curves, or of the one with respect to the other. The former locus becomes a pedal commonly so called when one of the curves degenerates into a point.

From the reasoning used above it is evident that the pedal of two such curves is an mn-circular $2mn$-ic.

This may also be deduced from the formula for the orthoptic locus as follows: The two curves make up a curve of class $m+n$, whose orthoptic locus is the aggregate of the pedal and the orthoptic loci of the two curves. The pedal is therefore of the order

$$(m+n)(m+n-1)-m(m-1)-n(n-1),$$

that is to say, it is of the order $2mn$, and it contains I and J as points of the order mn.

3. *Isoptic Loci and their Reciprocals.*

a. Any two tangents to a curve from I or J may be regarded as intersecting at angles α and $\pi-\alpha$ or these reversed, and their point of concourse thus belongs doubly to the corresponding isoptic locus. The order of such loci is therefore double of that of the orthoptic locus, and they pass twice as often through I and J.

For example—

(1.) The ellipse, to which one pair of tangents only can be drawn from I or J, may be regarded as subtending any angle or its supplement at those points. These are therefore double points on the corresponding isoptic locus, which is accordingly a bicircular quartic.

(2.) The parabola may be regarded as subtending any angle or its supplement at every point on the line at infinity. Its isoptic loci therefore contain the factor IJ^2, and the remainders, when this factor is rejected, are hyperbolas (or ellipses).

(3.) It may be deduced from the formula for isoptic loci, or proved directly by the method used above, that the skew pedals of a pair of curves of class m and class n respectively are $2mn$-circular,

$4mn$-ics. Thus the skew pedals of an ellipse with respect to a point (regarded as a curve of the first class) are of the eighth order, each consisting, of course, of two equal curves similar to the right pedal.

By taking a pair of lines drawn at random through either circular point, which may be regarded as inclined at an indeterminate angle, and supposing them to coalesce, we infer that any straight line through I or J may be regarded as making an indeterminate angle with itself.*

Hence the points of contact of the tangents from I and J to any curve are points on its orthoptic locus, and they are doubly points on its isoptic loci.

In the case of the conics these are the only points in which such loci meet the curve. If, therefore, $U \equiv \phi(x, y)=0$ be a conic, and $u=0$ its orthoptic locus, the bicircular quartics which are its isoptic loci will be represented by

$$U - k \,.\, u^2 = 0,$$

where k is a constant which vanishes when the isoptic angle is zero, in which case the locus consists of the conic and the line at infinity, and is infinite when the angle is a right angle, the isoptic being then the orthoptic locus.

Reciprocally, in a curve of the nth order, if a chord subtends a constant angle at a fixed point its envelope is of the class $2n(n-1)$.

The various points in the theory of plane orthoptic and isoptic loci propounded in this note have been verified by analytical methods in an unpublished paper by Mr. J. S. Yeo, Fellow of St. John's College.

The following notes on isoptic and other loci in space are taken from a valuable and suggestive series of investigations by Mr. Joseph Larmor, Fellow of St. John's College.

A solid has in general *six* degrees of freedom to move. The corner of a cube whose three faces are constrained to touch a surface loses three and retains *three*, and the locus of a point rigidly connected with it is not a surface, but a solid bounded by a certain envelope. When the cube-angle envelopes a quadric it can enjoy one of its degrees of freedom without displacement of its vertex, for if a cone of the second degree has one triad of orthogonal tangent planes, it has a

* It is sometimes said that such lines are at right angles to themselves; but this statement, although true so far as it goes, is inadequate. The angle between lines parallel to $y+mx=0$ and $y+m'x=0$ is $\tan^{-1}\dfrac{m\backsim m'}{1+mm'}$, and when $m=m'=\sqrt{-1}$, it is $\tan^{-1}\dfrac{0}{0}$, the numerator as well as the denominator vanishing.

singly infinite number: consequently the locus of the vertex contracts in this case into a surface, the orthoptic sphere. The locus of the vertex of a trihedral angle which envelopes a quadric is in general the space bounded by two surfaces.

Next consider a complex of lines of the nth order; those of its lines which pass through a specified point form a cone of the nth order; this cone can be circumscribed by a cube-angle provided the point lies within a certain solid space. When the complex is of the second order the solid locus degenerates into a surface, which is a quartic passing through the imaginary circle at infinity; and when the complex is composed of the tangent lines of a surface of the second order the locus is made up of the surface and its orthoptic sphere.

Similar considerations hold for the locus of the point of concourse of a triad of tangent lines at right angles. When the complex is of the second order the locus degenerates into a quadric.

Mr. Larmor has briefly considered the problem of a surface constrained to touch three surfaces, deducing as a special case that the locus of the vertex of a cube-angle whose faces touch a quadric or three confocal quadrics is a sphere.

IX. "On a New Form of Voltaic Battery." By PAUL JABLOCHKOFF. Communicated by WARREN DE LA RUE, M.A., D.C.L., Ph.D., F.R.S. Received May 12, 1884.

The battery which I have the honour to bring under the notice of the Royal Society is one of high electromotive force, namely, about two and three quarter volts, and a single cell consequently decomposes water; it is very light and portable, and convenient for many purposes. The electro-positive element is sodium, the electro-negative element is either carbon, spongy platinum, copper, or other metallic gauze; no fluid is used in which to immerse the plates, but the atmospheric air which is always impregnated with more or less hygrometric moisture serves to set up the action of the battery by giving up sufficient moisture to wet the surface of the sodium, so that a very thin film of fluid (a solution of soda), is thus interposed between the sodium and electro-negative element, and the internal resistance is very small in consequence of the thinness of the film of fluid. The sodium is used in the form of plates, conveniently about a quarter of an inch thick, and the plates of carbon, of which one is placed on each side of the sodium, a little longer and about the same thickness as the sodium; these plates, carbon, sodium, carbon, are kept together by means of vulcanised rubber bands, and suspended vertically, a vessel being placed underneath to receive the soda solution as it forms.

A battery composed of plates 10 inches long and $\frac{1}{2}$ an inch wide gives a current of 0·122 ampère at first starting, but as polarisation takes place, after five minutes, only 0·079 ampère. The cost of a battery of this size is 0·40 fr. (*4d.* about), it remains in action for six days without the renewal of the sodium. Batteries of larger dimensions, as for example 10 inches long and $1\frac{5}{8}$ inches wide, last four weeks, because the action is chiefly on the edges of the sodium plate, and the broader the plate the longer the sodium lasts without renewal.

X. "On the Electro-chemical Equivalent of Silver, and on the Absolute Electromotive Force of Clark Cells." By LORD RAYLEIGH, D.C.L., F.R.S., and Mrs. H. SIDGWICK. Received June 18, 1884.

(Abstract.)

The paper contains a record of a long series of experiments, extending over nearly two years.

The measurement of the electric currents is direct, not depending upon a knowledge of the force of terrestrial magnetism. Three horizontal coils are traversed in succession by the electric current. Of these two of large diameter are fixed, and at a distance apart equal to the radius of either. Symmetrically between them a smaller coil is suspended in the balance. When the current passes, the suspended coil is pressed down, or lifted up, according to the connexions, and the observations relate to the double force called into operation when the direction of the current in the fixed coils is *reversed*. In a paper read before the British Association at Southampton it was shown that this construction presents special advantages, and in particular that the calculation of the result does not require an accurate knowledge of the radii of the coils, but only of the *ratio* of the radii of the small and large coils. In this way one of the principal difficulties, the measurement of the small coil, is evaded.

The ratio of the radii is found by the electrical method of Bosscha. A large and small coil being adjusted so as to be concentric and coaxal, a very small magnet with attached mirror is suspended at the common centre. The two circuits are connected electrically in parallel, and resistance is added to one of them until no effect upon the suspended magnet follows a reversal of the battery current. The ratio of the resistances, to be found immediately by comparison with standards, is the ratio of the galvanometer constants of the two coils, and from this the ratio of the radii may be obtained by the introduction of small corrections relating to the finite dimensions of the

sections. Full particulars are given of the procedure adopted in the reduction of the method to practice.

The insulation of the small coil, which was wound upon a ring of ebonite, was carefully tested with the induction balance after the manner recommended by Graham Bell. The first attempt at winding it proved a failure, several turns being short-circuited; and we are of opinion that no coil of fine wire can be thoroughly depended upon which has not been tested by some such method.

The calculation of the constant of the current weighing apparatus is best made with the aid of elliptic functions. Both for our own purposes and in order to facilitate the use of the method by others, we have calculated a table of the function

$$\sin\gamma\,\{2F_\gamma-(1+\sec^2\gamma\,E_\gamma\},$$

(see "Maxwell's Electricity," 2nd edition, § 701), for values of γ ranging from 55° to 70°.

For determining the electro-chemical equivalent of silver, the current passes also through silver voltameters. The solution of nitrate, or of chlorate, is contained in a platinum basin which serves as the kathode. The anode is a flat piece of fine silver sheet, wrapped in filter-paper, and suspended by platinum wire at the top of the liquid. The duration of the current is determined by a chronometer, and allowance is made for the small loss of time (about one-tenth second), incurred at each reversal of the current in the fixed coils of the measuring apparatus.

In the preliminary notice of March, 1884, the troubles into which we were led by the use of acetate of silver were referred to. With pure nitrate the manipulations present no particular difficulty. We were equally successful with chlorate, prepared for us by Mr. Scott; and the comparison of the results with nitrate and chlorate verify Faraday's law to a high degree of accuracy.

In the reduction of the current weighings, we found it necessary to time all the observations, and to plot the readings obtained in the two positions of the reversing key as separate curves. The difference of ordinates then represents the double electromagnetic force, as it would have been found were it possible to take both observations simultaneously. What we require for comparison with the mean rate of silver deposit is the mean square root of the difference of weighings, and is easily obtained when once the curves are constructed. Apart from errors relating to the constant of the apparatus, the mean value of a tolerably steady current of half an hour's duration should be obtainable to about $\frac{1}{10000}$. In our experiments, the whole change of weight on reversal was about 1 grm., and each single observation was correct to half a milligram. In the passage from

the attraction to the current, the error is halved by the extraction of the square root.

The currents actually employed were about $\frac{1}{3}$ ampère. Much more powerful currents could not be passed for the necessary time through the suspended coil without risk of undue heating. Had it been desirable to use stronger currents, it would, of course, have been possible to do so by the use of thicker wire. With given grooves to be filled up, the ratio of the electromagnetic attraction to the heat developed is independent of the gauge of the wire; and the only further modification required would be the multiplication of the fine copper wires by which the flexible connexions are made.

Thirteen determinations of the ratio of the square root of the double attraction of the coils to the rate of silver deposit gave numbers ranging from 2413·7 to 2415·5, mean 2414·45; whence, after introduction of the constant of the apparatus, the value of the electrochemical equivalent in C.G.S. measure is found to be—

$$\cdot 0111794.$$

In terms of practical units, we have as the quantity of silver in grams deposited per ampère per hour—

$$4\cdot 0246.$$

With use of this number, it is now easy to determine by the deposit of silver currents up to $1\frac{1}{2}$ ampère. For currents of greater power it is necessary either to increase the size of the (3-inch) platinum basins serving as voltameters, or to dispose several such in multiple arc. For ordinary practical purposes, many of the precautions which we thought it necessary to adopt may be dispensed with. The deposits, after a few rinsings with distilled water, may be left to soak for an hour, and then, after a further rinsing, dried off over a spirit lamp. In an hour's time, the basin may be weighed correctly to a few tenths of a milligram. With regard to the materials, it is sufficient to use for the anodes a sheet of ordinary fine silver (such as is sold at 5*s.* per ounce), and a 15 or 30 per cent. solution of nitrate. It is hoped that this method may come into general use for the verification of current-measuring instruments, whose indications depend upon the constancy of springs, or of steel magnets. Silver presents so many advantages as compared with copper, that its greater cost should not stand in the way of its adoption, more especially as there need be no great waste of material.

In view of the importance of obtaining a convenient standard of electromotive force, a prolonged examination has been made of a number of Clark cells. Of these two patterns have been used, the first constructed according to the directions of Clark himself and of Alder Wright, with some simplifications; the second, called for

brevity the H-cell, in which the solid zinc is replaced by a fluid amalgam. The amalgam and the pure mercury, forming the metallic electrodes, are placed at the bottom of two small test-tubes standing vertically. The electric connexion is made by platinum wires sealed through the bottoms, and the communication between the two vertical tubes is through a lateral horizontal branch sealed into them. The cell is filled with sulphate of zinc solution above the level of the horizontal branch, and can then be closed with corks. This form of cell lends itself conveniently to experiment, as by withdrawing the corks it is easy to observe the effect of stirring or of various additions.

A large number of cells have been compared for more than eight months, and have behaved very satisfactorily. The results are sometimes anomalous for the first two or three weeks, but the values finally attained are in our experience extremely close together.

The comparisons are made by the method of compensation. The *difference* of electromotive force of the cells to be compared is compensated by a known fraction of the electromotive force of other cells, the value of which is then expressed in terms of one of the Clarks. There would be no difficulty in obtaining still greater sensitiveness, but it is useless to take readings closer than to $\frac{1}{10000}$.

The Clark cells possess the immense practical advantage (as compared, for instance, with Daniell's) of standing always ready for use, but the objection is sometimes expressed that they polarise greatly on the passage of the smallest currents. Our experience has been in the opposite direction, and has shown that moderate short-circuiting is actually advantageous in the case of cells newly set up. When old cells, which have reached their permanent condition, are allowed to make $\frac{1}{1000}$ ampère for a quarter of an hour, the disturbance thus occasioned passes off in about half an hour to within a few ten-thousandths, and on the next day there is no indication of any residual effect.

The absolute determinations of the E.M.F. of Clark's cells were made by compensation with the difference of potentials at the terminals of a known resistance traversed by a known current. The details of the method, which offers no special difficulty, are given in the paper. Of thirteen values, found on different days between October, 1883, and April, 1884, with the current measuring apparatus, the highest is 1·4552 and the lowest 1·4531. This number expresses the E.M.F. of a certain cell at 15° in terms of B.A. volts. To get the E.M.F. in true volts, the mean number (1·4542) must be multiplied by the number expressing the B.A. unit of resistance in absolute measure. If 1 B.A.=·9867 ohm, E.M.F. of Clark=1·435 volt.

The value of the H-cells would be a few ten-thousandths higher.

Apparatus capable of giving original determinations of the intensities of currents not being generally available, we have shown with

examples how by the use of the silver voltameter the E.M.F. of any cell can be found without much difficulty, and with scarcely any special appliances.

XI. "Preliminary Note on the Constant of Electromagnetic Rotation of Light in Bisulphide of Carbon." By LORD RAYLEIGH, D.C.L. Received June 18, 1884.

In connexion with other work upon current measurement by Mrs. Sidgwick and myself, we have endeavoured to determine the value of this constant, so as to decide between the very discrepant results arrived at by Gordon* and by H. Becquerel.† The method adopted by us was so far similar to that of Gordon that the tube of bisulphide of carbon was placed inside a helix, but the value of the current traversing the helix was determined in a different manner without reference to terrestrial magnetism.

The light employed was that emitted by sodium. When it is remembered that the effect would vary about two parts per thousand in passing from one sodium line to the other, the importance of definiteness in this respect will be obvious.

The number of turns in the helix is 3684, and the insulation was submitted to severe tests.

In carrying out the measurements the principal difficulty encountered was from optical disturbance arising from the communication of heat from the helix to the bisulphide. Not only does the mean temperature of the bisulphide rise somewhat rapidly during a series of experiments, but on account of the tendency of the warmer parts to find their way to the top of the tube, the light is sensibly diverted from its proper course. It is believed that by a modification of the apparatus about to be tried, this source of embarrassment will be materially checked.

The plane of polarisation was determined in some experiments by a Nicol read in two positions, and in others by a double image prism read in four positions. The adjustment of the match between the two parts of the field presented by the half-shade apparatus was facilitated by a device that may be found useful. In addition to the principal helix, the tube was embraced by an auxiliary coil of insulated wire, through which could be led the current from a Leclanché cell. This current was controlled by a reversing key under the hand of the observer, who was thus able to *rock* the plane of polarisation backwards and forwards through a small angle about

* "Phil. Trans.," 1877.

† "Ann. d. Chim.," 1882.

its normal position. The amount of the rocking being suitably chosen, the comparison of the three appearances (two with auxiliary current and one without) serves to exclude some imperfect matches that might otherwise have been allowed to pass.

On fifteen days sets of observations have been taken of the double rotation produced by the reversal of the current in the helix on light which traversed the tube *three* times. The double rotations varied from about 9° to 19°, and the currents from about $\frac{1}{2}$ ampère to 1 ampère. Reduced to correspond with a certain standard current, and corrected for temperature by Bichat's formula to 18° C., the double rotations ranged from 1124·1 minutes to 1132·2 minutes. The mean for 18° C. is 1128·4 minutes; and as this was about the actual mean temperature of the observations, the result is nearly independent of Bichat's formula for the dependence of the effect upon temperature.

Four sets of observations were also taken on light which traversed the tube but once. Multiplied by 3, and reduced to the same temperature and current, the mean of these gives 1127·4 minutes. In this case the current actually used was about $1\frac{1}{2}$ ampère, and the double rotation about 9°.

Taking both series of experiments into account, we may adopt 1128·0 minutes as the sixfold rotation at 18° C. due to the passage of the standard current through the helix.

In C.G.S. measure the value of the standard current is ·09722. The difference of magnetic potentials at points at infinity on the axis of the helix traversed by this current is

$$4\pi \times 3684 \times \cdot 09722.$$

The correcting factor on account of the finite length of the tube is ·99449.

Hence if x be the rotation in minutes at 18° C., corresponding to a difference of potential equal to unity, we have

$$1128\cdot 0 = 6 \times \cdot 09722 \times 4\pi \times 3684 \times \cdot 99449 \times x,$$

whence

$$x = \cdot 042002 \text{ minute.}$$

M. Becquerel gives as his result for 0° C. ·0463 minute. To find the rotation at 18°, this must be multiplied by ·9767 according to Bichat's formula: and as Becquerel's observations were in fact made at about 18°, this reduction does not introduce, but rather removes, an extraneous element. Thus according to Becquerel—

$$x = \cdot 0452 \text{ minute,}$$

differing by about 7 per cent. from the value found by us.

The comparison with Gordon is more uncertain, inasmuch as his observations were made on light of the refrangibility of the thallium line. The corrected* result for this light is in circular measure $1{\cdot}5238 \times 10^{-5}$, or ·05238 minute. To pass to sodium we may use a formula given by Becquerel† and Verdet according to which the rotation for different wave-lengths (λ) is proportional to $\mu^2(\mu^2-1)\lambda^{-2}$, μ being the refractive index. At this rate the ·05238 minute for thallium would be ·04163 minute for sodium. The temperature was not directly observed by Gordon, but was estimated to be about 13° C. Assuming this to be correct, the value for 18° would be ·0413 minute, or about 2 per cent. *less* than according to my determinations.

XII. "Certain Points in connexion with the Physiology of Uric Acid." (Supplemental.) By ALFRED BARING GARROD, M.D., F.R.S. Received June 19, 1884.

(Abstract.)

One of the objects of the present paper is the correction of an error of interpretation in the author's communication of February 15th, 1883. Another object is to make known certain facts which he has ascertained in seeking to correct the same.

The author hopes, 1st, to give a true explanation of the phenomena previously misinterpreted; 2ndly, to make known some facts hitherto unknown, whence arose the error in question; and 3rdly, to show the existence of certain peculiarities in the urine of herbivorous mammals, with regard to uric acid, which peculiarities the author anticipated, so that he was led to undertake numerous experiments and observations with the object of verifying or disproving such anticipations.

The subject discussed in Part I is the influence of alkaline carbonates upon uric acid. Many experiments are detailed, and, as a result it is found that weak solutions of the alkaline carbonates, when exposed to the air, possess the power of decomposing uric acid in solution, and that oxalic acid and urea are among the products of the decomposition. By the recognition of this fact certain physiological phenomena in relation to the urine can be explained.

Part II is devoted to the demonstration of the action of glycocine (glycocoll) and a few other substances in protecting uric acid from

* Mr. Gordon's result was originally given at double its proper value.

† "Ann. d. Chim.," t. xii, 1877, p. 78.

decomposition when in an alkaline solution. Glycocine is shown to have a very considerable power, even when in small quantities, such as $\frac{1}{10}$ per cent. of the solution. It was this fact which led to the misinterpretation of certain phenomena in the author's paper of 1883; for it was found that in a glycocine solution no change of uric acid took place; whereas, in a solution of hippuric or benzoic acid, decomposition readily ensued; and, as the influence of weak alkaline carbonates was not then recognized, the destructive action was attributed to these acids.

It will be seen that, in a free state, glycocine protects uric acid, but when in union with benzoic acid (in the form of hippuric acid) its protective power is lost.

The different protective powers of glucose and cane-sugar are alluded to.

Part III treats of the urine of the herbivorous mammal under different conditions. It occurred to the author that, if at any time the urine of such animals should lose its ordinary alkaline state and become acid, at such time uric acid would probably be found.

Twenty-five specimens of urine derived from nine different horses were examined; besides which many other specimens, from ten different horses, were tested, simply to ascertain the reaction of the urine.

The tests employed to demonstrate the presence of uric acid are fully described under each examination, and some of the more important characters of the urine are given.

In eighteen different horses the urine was found to be acid in two only, and even in these two the acidity of the urine was by no means constant, as each of them passed, at times, an urine which was fully alkaline in reaction.

Many conclusions are drawn from the above observations, among which it is found that the character of the food does not necessarily influence the reaction of the urine; neither does the state of health; it is also found that almost all the urines had oxalate deposits; most of them contained an abundance of calcium carbonate, and all the urines examined were rich both in urea and in hippuric acid.

Of the results brought out, by far the most interesting is that which has regard to the presence or absence of uric acid; for it is shown that there is an absence of uric acid from the urine of the horse, when the reaction is alkaline, as it ordinarily is, but on the other hand, it is equally proved that, when the urine of the horse exhibits an acid reaction, then uric acid is present in quantities equal to what is found in the urine of carnivorous mammals, as the lion and the tiger.

It is also shown that the presence or absence of uric acid does not

depend on any peculiarity in the animal, but simply on the reaction of its urine at the time.

The original view of the author, which led to his making the numerous observations above mentioned, which also he enunciated in his communication of February, 1883, appears to be fully verified by them; and the conclusion may fairly be drawn that, under ordinary circumstances, the absence of uric acid from the urine of the adult horse is not due to the non-formation of this acid, but to the destruction which it undergoes when formed, so that, if at any time the destructive influence is removed by the fluid assuming an acid reaction, uric acid appears in the urine.

Lastly, we may derive from the above observations an explanation of the fact that uric acid is present in the urine of the suckling herbivorous mammal, for in such animals the urine is found to be acid in reaction.

XIII. "A Redetermination of the Atomic Weight of Cerium." By HENRY ROBINSON, B.A., Assistant to the Professor of Chemistry in the University of Cambridge. Communicated by Professor LIVEING, F.R.S. Received June 11, 1884.

Having worked a good deal during the last few years on the preparation of pure compounds of the cerium and yttrium metals, I was led to seek some method by which I might obtain their anhydrous chlorides in such a state of purity that it would be possible to make from them redeterminations of the atomic weights of the metals. In April last year, I succeeded in preparing anhydrous cerium chloride (and in this paper I shall confine myself to that metal) by passing dry hydrochloric acid gas over what I may call air-dry cerium oxalate, heating it at first gently, so as not to char the oxalate, and then, as the operation proceeded, increasing the temperature to a full red heat. The chloride so obtained was perfectly white, and, when thrown into water, dissolved, with a hissing noise and a considerable evolution of heat, to a clear and colourless solution. I obtained the chloride so easily on the first attempt, I supposed I could repeat the experiment at will, but such did not prove to be the case, and, being much occupied with other duties, not much more was done towards the attainment of my object until the commencement of the present year. I had previously prepared a considerable quantity of pure cerium oxalate, in the following manner, from a crude yellow sulphate obtained from Schuchardt. It contained much didymium and

a smaller quantity of lanthanum, besides other metals. About 250 grms. of the crude material were taken at once, and the lumps were broken down in a mortar, then it was transferred to a flask of about 3 litres capacity and 100 cub. centims. of strong nitric acid were added to it, afterwards gradually water until the bulk was about 3 litres. After shaking frequently for a day, it was allowed to settle all night, and next morning the clear liquid was siphoned into another flask. Any undissolved was left in the flask to go on with the next lot, to be treated in the same way. The clear liquid which had been siphoned off was saturated with sulphuretted hydrogen, then filtered from the reddish-brown precipitate thrown down. To the filtrate oxalic acid was added, until there was no further precipitate formed; the precipitated oxalate was allowed to settle down, and the clear liquor was siphoned off and thrown away. The oxalate was then well washed with water made slightly acid with nitric acid on a filter, using a Bunsen pump, dried over a water-bath, and then ignited. The oxides thus obtained were dissolved in nitric acid, the solution was transferred to a dish and evaporated down on a water-bath to a thick syrup, until it would scarcely pour from the basin. It was then transferred to two large beakers, each containing 1500 cub. centims. of boiling dilute sulphuric acid—20 cub. centims. of sulphuric acid, sp. gr. 1·84 in 1000 cub. centims. of water—well stirred and allowed to settle. There was a considerable precipitate of basic cerium sulphate; after it had settled down well, the clear liquid was siphoned off, precipitated with oxalic acid, and the above process was exactly repeated to obtain more of the basic cerium sulphate. After siphoning off as much as possible of the clear liquid from the second precipitate, both precipitates were thoroughly drained together on a filter at a Bunsen pump, and washed with dilute sulphuric acid. The washings and the clear liquid from the second precipitate were precipitated with oxalic acid, and the oxalates obtained, principally of didymium and lanthanum, were reserved for further treatment.

Bunsen's process was now to dissolve the basic sulphate, and re-precipitate as such, repeating the process until he obtained a small quantity of the pure sulphate; but I found it better from this stage to adopt Gibbs's method of treatment with lead peroxide. Accordingly I dissolved the washed and drained basic sulphate in strong nitric acid, added to the solution some lead peroxide, and boiled the whole until a small quantity of the solution diluted with water gave no precipitate with barium nitrate. If properly done, the cerium was now peroxidised and in solution as nitrate. I now allowed the excess of lead peroxide and the lead sulphate formed during the process to settle down, then decanted the clear liquid into an evaporating basin, and heated it on a water-bath until it became a thick syrup. This syrup was treated with boiling dilute nitric acid—25 cub.

centims. of strong acid in 1000 cub. centims. of water—by which the cerium nitrate was decomposed into a basic nitrate which generally settled down readily, leaving only a small quantity of cerium in solution. The clear liquid was siphoned off and saved for further treatment. The basic nitrate was thrown on a filter, drained at the pump, and slightly washed with dilute nitric acid. It was removed from the filter-paper, transferred to an evaporating basin, and thoroughly heated on a water-bath until quite dry and hard, then broken up with a pestle in the dish, again set on the water-bath and heated, and, while there, drenched with boiling dilute nitric acid of the same strength as that used before. It was kept hot on the water-bath for some time and constantly stirred, then, after standing a short time, the greater part of the nearly clear supernatant liquor was poured away and the precipitate transferred to a filter, where it was drained by means of the pump and thoroughly washed with dilute nitric acid until some of the basic nitrate taken from the filter and dissolved in hydrochloric acid, so as to make a very concentrated solution, showed no trace of the didymium absorption-bands. When this point was reached the basic cerium nitrate containing a little lead was removed from the filter and put in a flask, hydrochloric acid was added, and the whole heated on a water-bath until the nitrate was converted into chloride and dissolved. The greater part of the excess of hydrochloric acid was evaporated away, as the oxalate of cerium, to be obtained subsequently, was found to be rather soluble when much of that acid was present. The cerium chloride was dissolved in water and the solution saturated with sulphuretted hydrogen, then filtered from the lead sulphide; the filtrate was boiled to get rid of the sulphuretted hydrogen, and again filtered to remove the small quantity of sulphur which separated out. The warm solution was then saturated with chlorine, and left to stand all night, so that if any iron was present it might be peroxidised. The cerium was then precipitated from the acid solution by well purified oxalic acid; the oxalate was allowed to settle down thoroughly, and the clear liquor was poured away. After several washings by decantation, using a dilute solution of hydrochloric acid containing about 2 per cent. of hydrochloric acid, the oxalate was transferred to a funnel which held a perforated platinum plate in its neck, and well washed with boiling water, then dried on the water-bath. All the cerium oxalate I have used in my experiments was prepared in the way just described. When I had obtained a sufficient quantity, the whole was well mixed in a mortar and then placed in a rather loosely stoppered bottle. From time to time a quantity sufficient for each experiment was taken from this bottle. When I had prepared all the pure oxalate I required, I hit accidentally on a simpler mode of separating cerium from lanthanum and didymium. Instead of precipitating the cerium

as basic sulphate, I evaporated the solution of the mixed nitrates of cerium, lanthanum, and didymium, obtained in the first instance, to complete dryness; then heated the brown mass over a naked flame until the brown colour entirely disappeared, and the residue became a pale yellow. On treating this yellow residue with boiling dilute nitric acid, the whole of the lanthanum and didymium was dissolved, and nearly all the cerium was left as basic nitrate.

When preparing the chloride I weighed out for each experiment about 10·5 grms. of the air-dry oxalate, and put it in a rather wide glass tube about 18 inches long. The tube was then placed in a paraffin-bath and dry hydrochloric acid passed through it. The hydrochloric acid gas was prepared in the following way:—20 parts of oil of vitriol were mixed with 8 parts of water, and the mixture was allowed to cool, it was then poured on to 12 parts of common salt. The gas is not produced when the acid is thus diluted until a gentle heat is applied, but when it is it comes so easily that it may be regulated to come at any desired rate. The gas was first passed through a Woulff's bottle containing oil of vitriol, and with three tubulures, the third one being connected with an apparatus for producing carbon dioxide, so that when necessary that gas could be admitted without disconnecting any part of the apparatus, next through two large U-tubes containing calcium chloride, then again through a bottle containing oil of vitriol, and lastly through a U-tube filled with asbestos to catch any spray of sulphuric acid which might be produced. The gas now entered the tube containing the oxalate, and the excess, after passing through a sulphuric acid wash-bottle, was absorbed in water. Soon after the hydrochloric acid began to pass over the oxalate, water made its appearance at the exit end of the tube. When the air was expelled heat was applied to the bath, much water continued to come off, and was driven away by heating the end of the tube. When the temperature of the bath rose a little above 120°—generally at 123°—the oxalic acid began to sublime to the end of the tube and condensed there in crystals, this in its turn was driven away by heating the tube. As long as oxalic acid was seen to come off the bath was kept between 120° and 130°, and all that did show itself as oxalic acid did so between these limits.

Afterwards the temperature of the bath was allowed to rise gradually to 200°; there was no advantage in going higher, as the paraffin evaporated away and a gas furnace can easily be regulated to this temperature. I now transferred the substance, not yet completely converted into chloride, to another tube and continued the process in an ordinary gas combustion furnace. The object now was to so regulate the temperature that water was kept coming off without increasing the heat so rapidly as to char the oxalate left

undecomposed. When a low red heat was reached a slow current of carbon dioxide was passed in along with the hydrochloric acid to burn the little carbon left from a small quantity of the oxalic acid nearly always charring. When the slightly grey appearance of the chloride due to this small quantity of carbon disappeared the current of carbon dioxide was stopped, that of the hydrochloric acid was continued for an hour longer, while the chloride was kept at a full red heat. With the hydrochloric acid still flowing over the chloride the furnace was gradually cooled until it was safe to handle the glass tube; the exit end of the tube was then plugged, then the other end, and the tube was removed from the furnace and wiped clean from the magnesia it had been lying in. The cork of the tube was then withdrawn and the chloride slipped into the weighing flask which was securely closed by a glass cap well ground on to its neck. It was then quickly weighed while still quite warm. The object in weighing the chloride at this stage in the process was to make quite sure no water was absorbed before the final weighing was made, and such was found to be the case, for although there was always a small increase in weight, about 0·0025 grm. on 6·5 grms. of the chloride, it was no more than was easily accounted for by the loss that would arise from weighing the body warm. When weighing the chloride it was placed in a small flask with a closely fitting cap and an exactly similar flask was used as a tare; the latter required the addition of 0·0005 grm. to balance the weighing flask; whatever was done to the one flask was always done to the other, and they were always kept beside one another. To avoid repetition, I may say here that in all the other weighings the body to be weighed was placed in one vessel and another vessel as similar in material and weight as possible was used as a tare. The weight having been taken, in order to remove extraneous hydrochloric acid, the flask containing the chloride, and the tare, were placed over sulphuric acid and surrounded by quicklime under the receiver of a large air-pump which could be exhausted very rapidly to within 3 millims. of a vacuum. This was usually done during an afternoon, they were left here the whole of the two following days, and on the morning of the fourth day air, dried by passing through sulphuric acid and phosphoric anhydride, was let into the receiver. This treatment was found to completely free the chloride from adhering hydrochloric acid, as when it was dissolved its solution was perfectly neutral. The two flasks were now removed from the receiver, securely closed, and the final weighing of the chloride made; it was then cautiously dissolved in water.

In making the following determinations pure silver was used prepared, according to Stas's directions, by precipitation from an ammoniacal solution of the nitrate by ammonium sulphite. For each experiment, after making the first weighing, I calculated how

much silver was required to precipitate the chlorine of the cerium chloride, assuming at the outset the atomic weight of cerium was 141·0. I then weighed in a porcelain crucible that quantity, less about 10 mgrms., and ignited it in a current of dry and purified hydrogen, keeping it some time at a red heat, allowed it nearly to cool in hydrogen, and then heated it again nearly to redness in air. After cooling over sulphuric acid the silver was weighed and transferred to a 20-oz. bottle, and the empty crucible was again weighed. A sufficient quantity of nitric acid, sp. gr. 1·42, was added to the silver, and the stopper of the bottle, which had been previously well ground in with emery, was tied down fast. The bottle was then placed in a water-bath, and the water was heated gradually until it boiled. It was kept at this temperature nearly an hour. There was no escape of gas at the stopper; if the stopper ever did slip the silver being dissolved was rejected, and a fresh quantity was weighed. The solution of the silver was made some time before it was required, so that the bottle and contents were quite cool when opened. After carefully cleaning the neck and stopper of the bottle on the outside the stopper was loosened and washed down into the bottle, a little more water was added when the silver was found to be completely dissolved, and the solution to be perfectly clear. The succeeding operations were performed in a room from which daylight was excluded. The solution of cerous chloride was poured into the solution of silver nitrate in the bottle in which it was prepared, and the whole was well shaken. The additional quantity of silver required for the complete precipitation of the chlorine was added by means of an approximately centinormal solution of silver nitrate, 1 grm. of which contained ·001064 grm. of silver. As I did not measure the quantity of the solution used but weighed it, I discarded the ordinary forms of burette in favour of a convenient sized bulb which I fused on to a glass stopcock; in this shape it was more easily weighed. The titration was made in an oblong box, divided into two compartments by a partition. The inside of the box was well blackened. In the partition a round hole was bored $1\frac{1}{4}$ inch in diameter; in the right-hand compartment a lamp was placed, and between it and the hole in the partition a spherical flask containing a solution of potassium chromate, so that the light from the lamp before passing through the hole had to pass through the yellow liquid in the flask. In the left-hand compartment the bottle containing the chloride was placed in such a position that the upper portion and surface of the liquid it contained was illumined by the pencil of yellow light proceeding through the hole. The burette was fixed in a stand in the box, so that it dropped the solution of silver nitrate into the bottle when the latter was in the best light. By this arrangement the slightest turbidity produced in the liquid by the silver

chloride is at once detected. The credit of originating it is due to Stas. With a little practice it soon becomes easy by observing the amount of turbidity produced by each addition of the silver nitrate to the chloride to know how many drops may be added the next time with safety, and as one drop of the solution of silver nitrate I used contained only $\frac{1}{20}$ of a mgrm. of silver there was not much danger of adding an appreciable excess.

I have made seven determinations of the chlorine in cerous chloride by the method I have just described. The mean of these determinations gives 139·8584 as the atomic weight of cerium when the atomic weight of hydrogen is taken as 1; if that of oxygen is taken as 16 then the number for cerium becomes 140·2154.

I have taken the specific gravity of cerous chloride by weighing it in carefully purified benzene, and I find it to be 3·88 compared with water at 15°·5. Then making corrections for weighing in air for both the brass and platinum weights and for the cerous chloride, I find the atomic weight in vacuo when hydrogen is 1 to be 139·9035, and when oxygen is 16 to be 140·2593. I thus find the atomic weight of cerium to be lower than Bührig did, but I defer making comments on the work of others who have preceded me until I have made some determinations of cerous bromide. This latter compound I have already succeeded in making by passing hydrobromic acid over cerium oxalate.

The ratios I have employed in this paper are those of Stas, and are as follows:—If the atomic weight of hydrogen is 1, that of oxygen is 15·96, silver is 107·66, and chlorine 35·37.

The atomic weight of oxygen being 16, that of silver is 107·93, and of chlorine 34·457.

The table below shows the quantities of silver and cerous chloride used in each experiment, and the ratios obtained.

No. of experiment.	Silver used.	Cerous chloride used.	Ratio of cerous chloride and cerium to 107·66 of silver.		Ratio of cerous chloride and cerium to 107·93 of silver.		The atomic weight of cerium.	
	grms.	grms.	$CeCl_3$	Ce	$CeCl_3$	Ce	Ag=107·66	Ag=107·93
2	7·26630	5·5361	82·0248	46·6548	82·2309	46·7733	139·9644	140·3205
3	7·98077	6·0791	82·0066	46·6366	82·2110	46·7540	139·9098	140·2620
4	8·50626	6·4761	81·9652	46·5953	82·1710	46·7140	139·7856	140·1420
8	9·18029	6·98825	81·9533	46·5833	82·1614	46·7044	139·7499	140·1132
9	8·78015	6·6873	81·9968	46·6268	82·2036	46·7466	139·8804	140·2398
10	9·20156	7·0077	81·9914	46·6214	82·1970	46·7400	139·8642	140·2200
11	9·13930	9·6600	81·9881	46·6181	82·1937	46·7367	139·8543	140·2101

XIV. "The Relation between Electric Energy and Radiation in the Spectrum of Incandescence Lamps." By Captain ABNEY, R.E., F.R.S., and Lieut.-Colonel FESTING, R.E. Received June 6, 1884.

In the "Philosophical Magazine" for September, 1883, we showed that certain relations existed between potential, current, watts (volt-ampères), and radiation in incandescence lamps, and that if p=the potential, c= current, w=watts, R=radiation, and a and b were constants, then

$$\text{(i)}\quad c=ap+bp^{\frac{3}{2}},$$

and consequently that

$$\text{(ii)}\quad w=p^2(a+bp^{\frac{1}{2}})\,;$$

further we showed that after the carbon filaments attained a certain heat, the radiation varied directly as the energy.

$$\text{(iii)}\quad \mathrm{R}\propto(w-m)\ \text{where } m \text{ is a constant.}$$

We may incidentally mention that this relationship appears to hold good in a properly exhausted lamp, be the filament of carbon or of platinum, or presumably of other metals. In this case the sign in the left hand members of equations (i) and (ii) is changed.

On laying down graphically the curve obtained by using the watts as the abscissæ and the radiation as ordinates, it was at once evident that the straight part is an asymptote to a curve having its origin at 0. It might, therefore, be presumed that each individual ray should increase in intensity in somewhat the same manner when the energy in the filament was increased, *i.e.*, in some simple curve which would have an asymptote.

The result of our researches which we now communicate to the Royal Society is that the curves for rays of refrangibility lower than about $\lambda 8500$ are hyperbolas, and that when near the limit of visibility the hyperbolas approach the parabolic form, the origin of the curves moving away from the zero of energy as the rays are more refrangible. This may be put in the form of an equation—

$$(w-m)^2=k\mathrm{R}^2\pm l\mathrm{R}.$$

When the rays are of very low refrangibility we have the − sign in the equation, "l" gradually diminishes as the refrangibility of the rays increases till it becomes 0, with a still further increase it takes the + sign; "k" gradually diminishes from the greatest wave-length till it approaches 0, after which the curves practically become parabolas. A reference to fig. 2 will show the forms of the curves for different

parts of the spectrum. The following Table I will give an idea of the alterations in l and k. The observations were made a year ago on a Lane-Fox lamp.*

Table I.

Watts.	Ray 1. Observed.	Ray 1. Calculated.	Ray 2. Observed.	Ray 2. Calculated.	Ray 3. Observed.	Ray 3. Calculated.	Ray 4. Observed.	Ray 4. Calculated.
11·80	..	..	3·5	3·4	5	5·4	5·5	5·9
20·05	1	1·3	9·0	9·0	13	13	12	12·2
26·10	2	2·07	14·5	14·2	18·5	18·8	17·0	16·7
39·40	5	4·9	27	27·6	..	..	27·5	27·3
46·61	7	7	36	35·4	39·5	39	34·5	33·4
60·29	11·5	11·5	51	51	53	52·8	..	..
71·95	16·5	16·4	64	64·5	64·5	64·3	55	54·3
87·97	24	24·6	83·5	84·1	80·5	80·8	67·5	67·2
100·50	32	32·1	99	99·3	93	94	77	77·4
106·30	36	36	106·5	106·5	..	..	..	..
112·80	41	40·3	114·5	114·5	106	106	88	87·6

No. 1 ray is the limit of the visible red, and is approximately parabolic, 1·8 watts having to be deducted to arrive at the origin of the curve, and l=311·2.

In No. 2 $\begin{cases} l=38{\cdot}8. \\ k=\ \ {\cdot}632. \end{cases}$ In No. 3 $\begin{cases} l=18{\cdot}3. \\ k=\ \ {\cdot}959. \end{cases}$ In No. 4 $\begin{cases} l=15{\cdot}7. \\ k=\ 1{\cdot}48. \end{cases}$

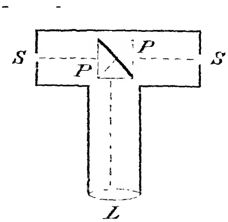
It will be noticed that in the curves of rays 2, 3, and 4, the movement of the origin from zero of energy must be very small, and is therefore negligible; hence for all the hyperbolas, which differ appreciably from parabolas, we may take the zero watts for the origin of the curve.

We further give tables showing that the results are true with the visual rays as measured by ordinary photometric methods.

Table II shows the calculated and observed intensities of three visible rays compared with the same rays in a gas flame. The method of observation adopted was by means of a spectrophotometer which one of us has used during the last ten years. It consists of two slits, SS, through which the light from the two sources which have to be compared respectively pass, striking two right angle prisms, P, superposed over one another as shown in fig. 1. These

* Table XIII is a still more complete record of the alterations in k and l, and may be studied in connexion with this.

FIG. 1.



reflect both beams on to a collimating lens L, whence they fall on a dispersion apparatus, two spectra being formed on a screen, or in an observing telescope. A slice of the two spectra is taken and compared side by side by means of a "split" lens. The intensity of either spectrum can be reduced by closing one of the slits, or by placing a rotating disk of sectors between the slit and the screen, and adjusting the opening of the sectors till equality of illumination is attained. The above results were obtained by the latter mode of observation.

Table III shows a ray selected from the spectrum whose intensity was also compared with a ray of the same wave-length in the spectrum of a gas flame. Tables IV, V, VI, and VII show the readings made photometrically, through red and green glasses, of different lamps, as compared with a candle-power, the observed and calculated intensities being shown in juxtaposition. It will be seen that the coincidences are very close, and seem to justify the conclusions we have drawn. Table VIII is an attempt to measure the light as a whole from the incandescence lamp with that of sperm candles. The results are as near as might be expected.

Table II.

Watts.	λ 6600.		λ 5550.		λ 4700.	
	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
67·5	31·7	31·7	36·7	36·7	38·5	38·5
57·4	20·5	20·3	24	23·8	22·5	22·7
46·1	14·0	14·0	13	12·7	11	10·9
37·6	9·0	8·9	— 7	7·0	4·75	4·75
21·9	2·7	2·7	— ·5	·6	..	..
11·4	·5	·5	..	..	..	..

Table III.—Maxim Lamp.

Watts.	Red ray.	
	Observed.	Calculated.
38·9	1	1
56·9	5·5	5·5
77·9	13	13
102·5	27·9	26·6
129·5	52·2	49·2
160·9	81·5	80·7
245	202	206
282	276	280
316	359	359

Table IV.—Maxim Lamp.

Watts.	Red candles.		Green candles.	
	Observed.	Calculated.	Observed.	Calculated.
24·3	·4	·4	·3	·3
39·3	2·2	2·2	2·2	2·2
52·2	5·9	5·9	8·9	7·0
78·4	17·2	17·2	21	18·9
104·4	34·2	34·2	40·8	40·9
133·2	59·5	59·5	74·4	74·4
165·2	98·8	96·1	122·0	126·0

Table V.—Lane-Fox.

Watts.	Red candles.		Green candles.	
	Observed.	Calculated.	Observed.	Calculated.
50·4	1·0	1·0	·77	·7
79·2	5·9	5·9	5·0	5·0
117·2	17·9	18·3	21·9	22·1
158·4	40·4	40·8	57·0	56·4
205·0	74·4	74·4	114·1	114·1

Table VI.—Edison Lamp (Six Candles).

Watts.	Red candles.		Green candles.	
	Observed.	Calculated.	Observed.	Calculated.
15·83	·33	·3	·25	·25
27·12	1·8	1·8	2·0	2·2
38·45	5·2	5·13	6·0	6·0
54·41	13·5	12·9	21·0	21·7
73·5	26·5	26·9	44·8	44·8
93·0	46·5	46·5	79·7	79·4
119·7	79·7	79·2	142·1	142·0

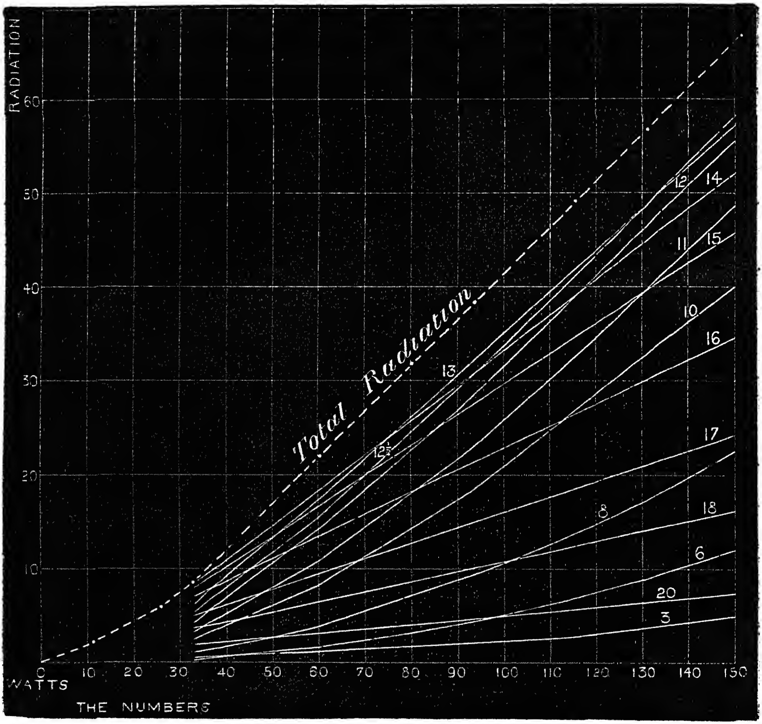
Table VII.—Edison Sixteen-Candle Lamp.

Watts.	Red ray.		Green ray.	
	Observed.	Calculated.	Observed.	Calculated.
29·2	·6	·4	·5	·4
37·5	1·6	1·5	1·6	1·6
47	3·3	3·4	4·0	4·0
57·7	6·5	6·4	8·0	7·7
71·1	11·8	11·4	15·3	15·3
84·3	18·3	18·1	24·3	24·3
99·1	27·9	27·2	35·7	37·9
114·1	38·8	38·5	54·5	52·5
132·2	54·5	54·3	74·4	74·4
151·7	74·4	74·4	103·0	102·2

Table VIII.—Woodhouse and Rawson's Lamp.

Watts.	White light.	
	Observed.	Calculated.
106·1	5·2	5·2
93·6	11·2	9·8
75·5	21·6	22·3
63·5	35·5	35·5
48·5	56·4	61·5
40·5	84·5	84·3

FIG. 2.



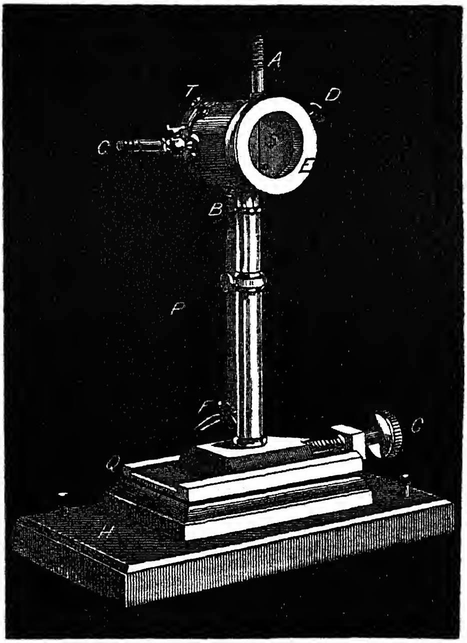
Note.—The numbers attached to the unbroken curves indicate the turns of the screw of the pile which define the parts of the spectrum to which the curves respectively refer. The wave numbers of these can be found by a reference to Table X.

We now come to the method we adopted in measuring the radiation at different parts of the spectrum.* A collimator with a lens of the same glass as the prism was used to render the rays from the incandescence lamps parallel before falling on the prism. Another lens of the same material, fitted on to a camera with horizontal swing back, received the spectrum which was focussed on the screen of the latter. A linear and very delicate thermopile mounted on a stand with a screen was employed; the accompanying figure will give an idea of the instrument (fig. 3). S is a polished silver slit placed in front of the linear couples, which could be closed at will by a screw D. E is a brass mount for a plate of glass or rock-salt, a similar one being

* This description was added after the paper was read, as we were advised that the subject-matter would thereby be rendered more clear. (July 14.)

fitted at the other side of the pile. C is a tube to be connected with a pump for exhausting the chamber in which the couples are placed, the ends of which are the glass or other plates. There is outside this chamber another hollow chamber through which a current of water can be passed, the intake being by B, which is connected by an india-rubber tube P to the water supply. F is an ordinary pinchcock. G is a screw of 30 turns to the inch by which the pile can be moved along the guides QQ. The base is let into a wooden block which is screwed down to the observing table. It will be seen that the height

Fig. 3.



of the pile can be adjusted. TT, one not shown in the figure, are the terminals which are connected with the pile and the galvanometer. The face of this thermopile was placed in the spectrum and moved along it, the deflections of the galvanometer being noted at the different parts for the different currents passed through the lamp. The slits were adjusted so as to be approximately equal to one another, and were kept of an aperture of about $\frac{1}{50}$ inch. The galvanometer used was one of very low resistance, and set up so as to be exceedingly sensitive. The currents and potentials were measured by Thomson's graded galvanometers, but in some cases the

Table IX.

Watts.	Turns of the screw.	0		3		6		8		10		11		12		12½		13		14		15		16		17		18		20	
	Total radiation. Deflections.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
150·5	66·5	3	...	5	5	12	12	22·5	22·5	40·5	41	49	49	56	56	58	58	56·7	57	52	52	46	45·3	34·5	34·5	24	24	16	16	7	7
130·8	56·5	1	...	...	3·75	9	9	17	17	33	32·5	40	39·3	46	46	48	48·2	48·5	48·1	45	44·0	39	39	30	29·8	21	20·8	14	13·9	6·5	6·3
116·4	49·0	...	...	3	3	...	...	...	13·7	27	26·8	33	32·6	39	39·2	40·5	41·3	41·7	41·7	38·5	39·2	34·5	34·5	26·5	26·6	...	18·6	...	12·4	...	5·5
93·8	38·0	...	...	...	...	5	4·0	9	9	18·5	18·2	22·5	22·7	28·5	28·3	30·0	30·2	32·0	31·5	30·5	30·6	27·0	27·1	21·0	21·2	15	14·9	11·0	10·0	5·0	4·7
60·2	21·0	...	...	...	...	1·5	1·9	3·75	3·75	8·0	8·0	10·5	10·2	14·0	13·8	15·5	15·3	17·0	17·0	18·0	18·0	16·5	16·6	13	13·2	9·5	9·5	6·5	6·5	3·0	3·0
33·6	8·0	...	...	·5	·5	...	...	1·0	1·1	2·5	2·6	3·5	3·4	5·0	4·9	6·0	5·6	7·0	6·8	8·0	8·4	8·0	8·0	7·0	7·0	4	5·2	3·5	3·7	1·5	2·0
20·7	5·5																														
11·0	1·5																														

These columns indicate the radiation for the several watts at different turns of the screw. The observed and calculated values of the radiation are shown in parallel columns.

potential was read by one of Siemens' volt-meters. In every case the potential was taken close to the lamp. Grove's cells were used to obtain the necessary currents.

The unbroken lines in fig. 2, as we have already said, show the forms of the curves at different parts of the spectrum. The light used in this experiment was from one of the British Electric Light Company's lamps.

The fiducial energies measured were 150·5, 130·8, 116·4, 93·8, and 33·6 watts. From these the hyperbolas were calculated. Table IX gives the calculated and observed deflections, from which figs. 3 and 4 were constructed, and also the total radiation as observed on the thermopile. The observed and calculated radiations for different parts of the spectrum agree very closely.

The wave-lengths corresponding to the position of the thermopile at different turns of the screw in this case were as follows:—

Table X.

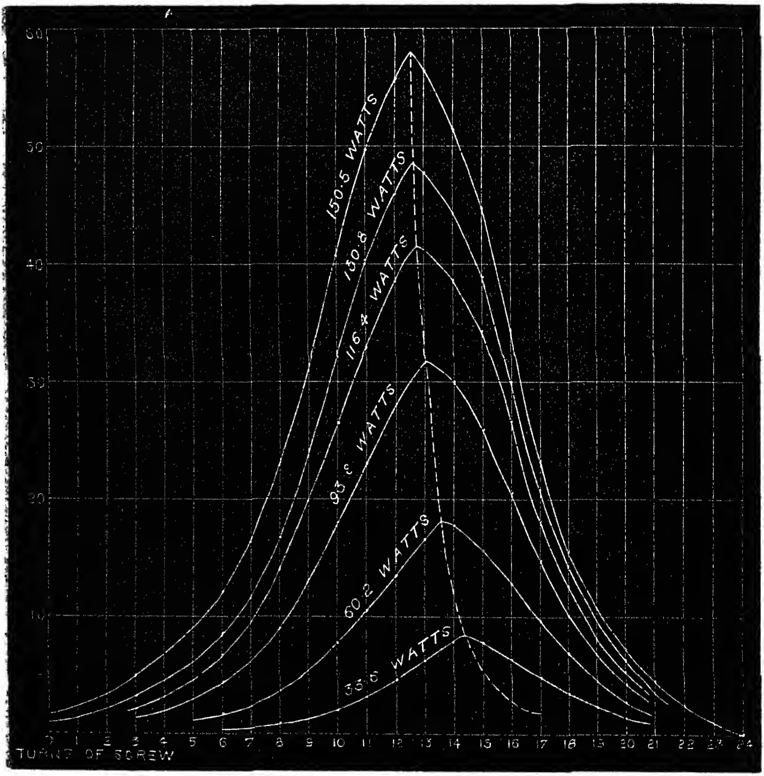
Turns of screw.	λ	Turns of screw.	λ
0	5,900	13	13,200
1	6,150	14	14,300
2	6,450	15	15,500
3	6,700	16	16,700
4	7,050	17	17,900
5	7,400	18	19,150
6	7,800	19	20,400
7	8,250	20	21,700
8	8,800	21	23,000
9	9,500	22	24,300
10	10,300	23	25,700
11	11,200	24	27,100
12	12,200	25	28,600
12½	12,700		

These, as well as the wave-lengths shown in the other tables, were obtained by taking photographs of substances giving lines which have previously been mapped. The back of the pile was covered with thick felt, and kept at a constant temperature by means of ice. A screen was placed in front of the slit. No radiation hence fell on the apparatus except for the instant required to cause the needle to swing.

In fig. 2 the dotted line shows the results obtained by measuring the total energy of radiation from the lamp. It will be seen by a reference to our paper ("Proc. Roy. Soc.," vol. 35, p. 328) of 1883, that the energy of radiation for the visible spectrum of incande-

scence lamps as measured by the thermopile is very small compared with that of the dark rays, in fact is almost negligible, and from fig. 2 it will be seen that after a very few watts of electrical energy, the intensities of the rays which radiate most energy increase directly proportionally to the number of watts expended, and hence that their integration is also directly proportional to the number of watts expended.

FIG. 4.



Further for any slight deviation from the straight line in the most refrangible rays there is a deviation in the opposite direction in the rays of very low refrangibility. These apparently counterbalance one another, and hence we have the total radiation becoming a straight line.

It now becomes of interest to determine the thermogram of the radiation for different electrical energies expended in the filament, and such are given in fig. 4.

Table XI.—Woodhouse and Rawson Lamp.

Turns of screw	0.		3.		6.		7.		8.		9.		10.	
Watts.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
132·0	0	...	5	...	18	18	27	27	39	39	53	53	67	67
106·5	...	...	3	...	11	11·1	17·5	17·2	25	25	35·5	35·8	48	48
70·0	...	...	1·5	...	4	4	7·0	7·0	11	10·8	16	16·2	24	23·3
40·5	...	...	·5	...	1	·9	2·2	2·0	3·5	3·5	5·5	5·5	8·5	8·5
18·0	...	...	...	...	...	...	...	...	...	...	1·0	·8	2	1·8

Turns of screw	11.		11½.		12.		13.		14.		15.		16.		17.		18.		19.		20.	
Watts.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.	Observed.	Calculated.
132·0	77	77	80·5	80·5	78·5	78·8	68	68	53	53	36	86	23·5	23·1	14	14	7	...	3	...	·5	...
106·5	57	57·2	60·5	60·5	59·5	60·2	53	53·2	42	42·1	29	28·9	19	19	11·5	11·4	5	...	1·5	...	...	...
70·0	30·5	30·3	32·5	32·3	34·5	34·5	32·0	32·2	27	26·5	18·7	18·8	13	12·7	8	7·9	3	...	...	...	...	...
40·5	11·7	12·0	13·7	13·7	15·5	15·2	16·0	15·7	14	14	10·75	10·8	7·5	7·6	5	4·9	2	...	...	...	...	...
18·0	2·5	2·6	3·0	3·1	4·0	3·8	4·5	4·5	5	5	4·5	4·5	4·0	4·0	3	3	...	...	...	...	...	...

Table XII.—Showing the wave-lengths for the turns of the screw in Table XI.

	λ			λ
1·25	= 5,900	....	12	=13,300
3	= 6,800	....	13	=14,650
5	= 7,250	....	14	=16,300
6	= 7,700	....	15	=17,650
7	= 8,250	....	16	=19,150
8	= 9,000	....	17	=20,750
9	= 9,900	....	18	=22,350
10	=10,950	....	19	=24,050
11	=12,100	....	20	=25,700
11½	=12,700	....	21	=27,500

The radiation from different carbon filaments gave almost identically the same results, whether the surface was bright or whether the filament was dead black, a result for which we were not quite prepared. For owing to the kindness of Messrs. Woodhouse and Rawson, who specially prepared a lamp, we were able to compare the two legs of a filament, one of which was bright and the other dead black, though the resistance per unit of length was the same, and the legs were in fact identical except in this respect. The bright leg of the loop radiated visible radiation much more freely than the dead black leg, and we therefore expected to find a larger proportion of dark rays in the spectrum. As before stated such was not the case, and we found that the bright filaments were able to stand a higher temperature than the dull filaments before giving out. Table XI shows the result with a filament having a metallic lustre, whilst that of which Table IX is a record was jet black. Tables XII and XIII give respectively the wave-lengths and the constants k and l of this lamp.

Table XIII.—Showing the constants k and l for the different turns of the screw in Table XI.

In formula $w^2 = kR^2 \pm lR$:—

Turns of screw.				
For 17	......	$l = -186$	......	$k = 103$
16	......	$l = -60$	......	$k = 35$
15	......	$l = +13$	......	$k = 13{\cdot}1$
14	......	$l = +38$	......	$k = 5{\cdot}5$
13	......	$l = +59$	......	$k = 2{\cdot}91$
12	......	$l = +80$	......	$k = 1{\cdot}80$
11½	......	$l = +99$	......	$k = 1{\cdot}465$
11	......	$l = +120$	......	$k = 1{\cdot}38$
10	......	$l = +183$	......	$k = 1{\cdot}145$
9	......	$l = +289$	......	$k = {\cdot}77$
8	deduct	0 watts and the curve calculated as a parabola.		
7	„	6	„ „	„
6	„	14·5	„ „	„

It seemed of interest to compare a platinum incandescence lamp with a carbon filament, and the results we show in Table XIV, and fig. 5.

FIG. 5.

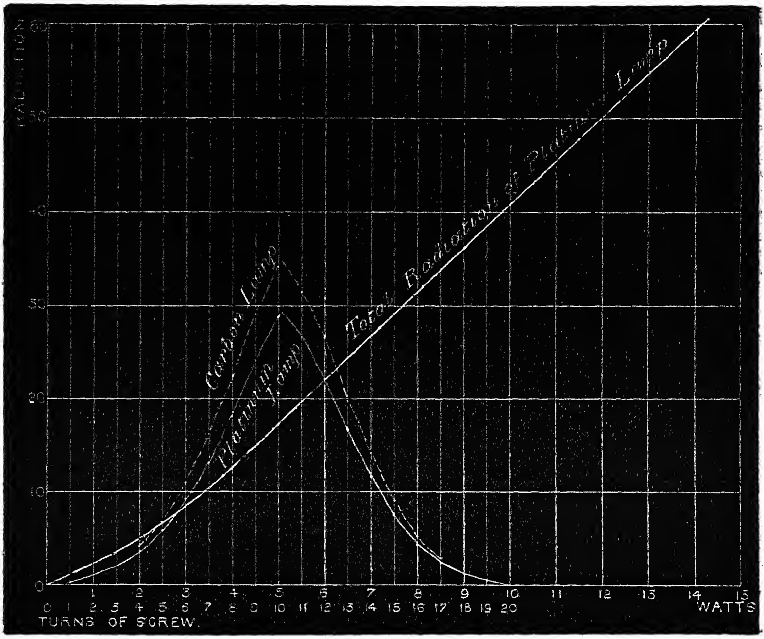


Table XIV.—Radiation of Platinum Wire Lamp (spectrum), watts =12·2, and also of Carbon Filament Lamp (spectrum), watts =70·1.

Turns of the screw of thermopile.	Wave-length.	Platinum wire lamp.	Carbon filament lamp.
0	5,800	0	0
2	6,900	1	1
4	7,800	3	3·5
6	9,300	9	10·5
7	10,250	13	16·0
8	11,300	18	21·5
9	12,500	24	29·0
10	13,800	28·5	34·0
11	15,150	26·5	32·0
12	16,550	22	26·5
13	17,950	16·5	19·5
14	19,350	11·5	14·0
15	20,800	7·5	9·0

Table XIV—*continued*.

Turns of the screw of thermopile.	Wave-length.	Platinum wire lamp.	Carbon filament lamp.
16	22,300	4·5	5·0
17	23,800	2·7	3·0
18	25,300	1·0	1·0
19	26,800	·5	·5
20	28,300	0	0

The radiation curves of the two spectra are similar in every respect, showing that the energies of the rays at different parts of the spectrum bear the same proportion in the two cases. We have already mentioned that the law that connects the volts and ampères in a current passing through a carbon filament applies to platinum wire. In Table XV we give the observed and calculated current

Table XV.—Platinum Lamp.

Volts.	Ampères observed.	Ampères calculated.	Watts.		Total Radiation.
2·0	7·1	7·1	14·2		60·5
1·75	6·65	6·6	11·55		48·0
1·65	6·35	6·35	10·48		42·5
1·5	6·0	6·03	9·04		36·0
1·4	5·75	5·74	8·04		31·5
1·3	5·45	5·48	7·22		27·0
1·2	5·15	5·17	6·21		22·5
1·1	4·9	4·88	5·37		18·5
1·05	4·65	4·72	4·95		16·5
1·0	4·55	4·55	4·55		14·5
·9	4·3	4·23	3·81		11·0
·8	3·95	3·84	3·07		8·5
·71	3·55	3·51	2·49		6·5
·64	3·20	3·23	2·08		5·0
·53	3·0	2·75	1·46		3·0
·42	2·65	..	1·15		2·0
·30	2·1	..	6·3		1·0

together with the total radiation observations, and it will be seen in fig. 5 that the curve becomes a straight line after the expenditure of about 3 watts of electrical energy. We may, therefore, conclude from these experiments that bright or dull carbon and platinum behave similarly to one another in every respect as regards quality of radiation.

We would again refer to fig. 4 in which the line passing through the maxima is produced below the last observed thermogram. This

production was effected by calculating the position of maxima from the hyperbolas calculated in Table IX. It will be noticed that the maxima rapidly recede towards the lower limit of refrangibility, and at just invisible heat the maximum lies near 18,000.

When a filament is at a constant temperature the energy required to maintain it at such must of necessity be expended, either by radiation, by heating matter in immediate contact with it, which in this case is gas in a state of extreme tenuity, by conduction, or by an internal loss.* If there be no expenditure of energy by anything except radiation, it is manifest that this would increase in exact proportion to the energy shown in the filament. Now we have seen that the radiation is not thus strictly proportional to the expended electrical energy, but that from the first we have energy unaccounted for by radiation, which gradually increases up to a certain point and then becomes constant. The question arises as to what cause this can be due. A reference to fig. 2 will show that convection currents either inside or outside the lamp are not the cause. We have in this figure the analysis of the radiation for different parts of the spectrum, and it will be seen that the rays of low refrangibility have curves which are concave to the axis of abscissæ, whilst for the rays of higher refrangibility the curves are convex to the same axis. Now if convection currents caused this palpable concavity in the curves from rays of low refrangibility, we ought to be able to measure their radiation as total radiation from the lamp when the direct radiation of the filament itself is cut off. As a matter of fact the total radiation from the globe and surrounding air was immeasurable on the thermopile. Thus the radiation from the heated gas inside, and the heated air outside the lamp, and from the glass globe itself, were inappreciable. The former might, perhaps, be expected if the lamp-black with which the face of the pile was coated did not absorb the particular radiations emitted from them. If the total radiation from the glass globe and its surroundings was not measurable, much less could it be anticipated that the galvanometer needle would be deflected when such radiation was distributed through the spectrum, more especially as the rays falling on the prism proceeded from a very narrow section of the lamp (including the filament) lying on a plane passing through the slit and along the axis of the collimator. We find, however, that in this case the curves of radiation of the rays of low refrangibility are concave, whilst those of higher are convex to the horizontal axis. This would imply, if the cause of the convexity of the total radiation curve lies outside the filament (since it has been shown that the radiation from the globe and surroundings is inap-

* Any apparent loss in radiation due to the lamp-black on the thermopile not absorbing radiation may be dismissed, as the loss will be shown to occur in the rays of high refrangibility which it is known are absorbed by lamp-black.

preciable), that the rays of high refrangibility are more absorbed or used up than those of low refrangibility, which is quite contrary to all our knowledge of the laws of absorption. We are, therefore, obliged to look to the filament itself for the cause of the convexity. We know that in a carbon filament resistance diminishes as the temperature is raised, whilst in a platinum wire the resistance increases under the same circumstances. In both cases this implies a re-arrangement of the molecules of the carbon or platinum and the consequent using up of energy. It seems that this might be the reason of the form of the curves. If it be so, then as the radiation from a filament varies as the surface, and the alteration in molecular arrangement varies as the mass, we ought to find that in excessively fine filaments the convexity is much diminished, and that the radiation curve is practically a straight line starting from the origin. This is found to be the case in some filaments as fine as those made by Edison. It should also be noticed that from the part where the radiation curve becomes straight, the resistance of the filaments changes but very slowly.

We have specially made these remarks, since in a paper read before the British Association at Southport, the late Sir William Siemens took exception to our regarding convection currents as being absent, and stated that these currents were imparted to the air outside the lamps. This objection is theoretically valid, but practically it is of no moment. We propose to examine this more fully subsequently.

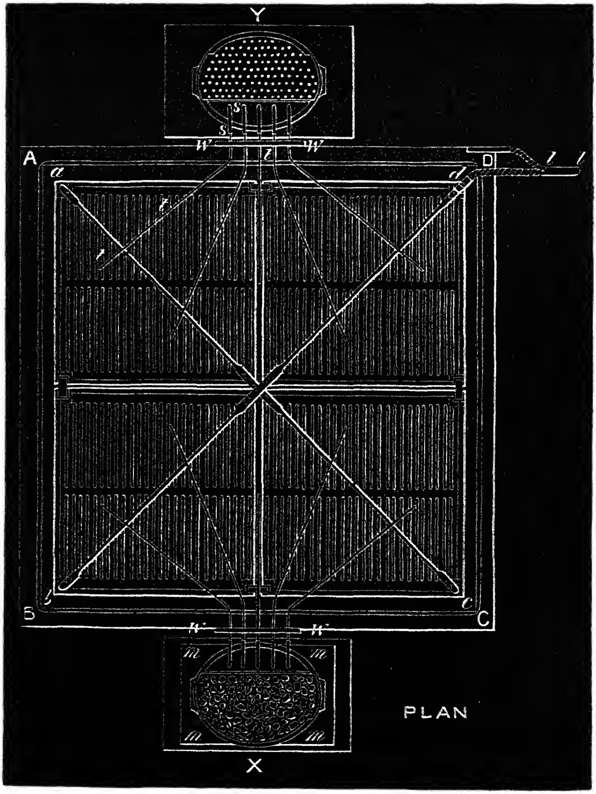
A point now arises in regard to this research which is of importance in photometric measurement. If the intensities of radiations of a ray, say in the red of the spectrum of an incandescence lamp as produced by two known electrical energies, are compared with the corresponding radiations (say) of a candle, and if then the intensities of a second ray, say in the green, are also compared, the number of watts necessary to produce any temperature in which the same red and green rays bear a known proportion to the same rays in a candle flame may be found by a simple calculation. If the thermopile be used, the same procedure may be adopted, in which case it would be advisable to use rays in the invisible spectrum of low refrangibility. For this, of course, the prism must be used with judgment, and after its constants have been properly determined, which we have found no difficulty ourselves in doing. With a grating the deflections on the galvanometer are much smaller, and the errors of observation are consequently likely to be proportionally larger than if the large deflections as given when using a prism are employed. Then, again, too, in using a reflection-grating, certainty must be obtained that the intensity of the spectra follows the theoretical law, or at all events it must be ascertained what is the deviation from it. This is a point that Professor Rowland has exemplified practically, as he has produced gratings in which the intensity of spectra on each side of

the central image are not comparable, and one of us has a Rutherfurd grating in which the infra-red rays in the 1st order are absent after λ9000. In conclusion we may say that the radiation apparently varies as to mq^{T}, where m and q are constants and T the temperature.

XV. "On a Gravity Daniell's Cell of very small Internal Resistance." By J. T. BOTTOMLEY. Communicated by Professor Sir WILLIAM THOMSON, F.R.S. Received May 29, 1884.

I beg leave to describe a new arrangement of gravity Daniell's cell which I have found manageable and convenient for supplying

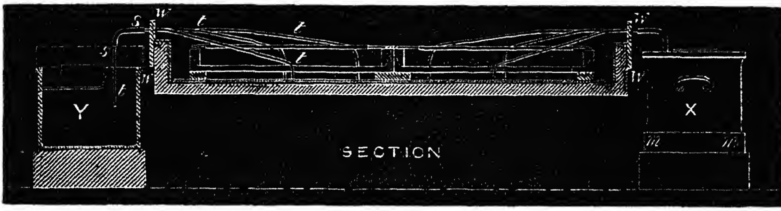
FIG. 1.



continuously and steadily a very powerful current of electricity. It has proved of great service for graduating ampere-meters and for such purposes.

Fig. 1 shows the cell in plan, and fig. 2 the same in section. The outer containing vessel is a large shallow tray of wood, very strong, but quite roughly made, and lined with sheet lead. The tray is

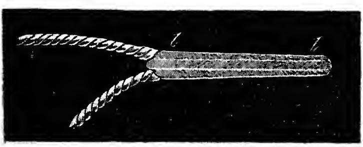
FIG. 2.



$3\frac{3}{4}$ inches deep, and with this small depth the lead can be beaten in, and autogenous soldering is unnecessary. The lead tray is very carefully varnished with spirit varnish, several coats being applied; and on the bottom of it is laid a thin sheet of copper (0·2 millim. thick), or rather several sheets thoroughly connected together, as I have not been able to obtain a single sheet of copper large enough for my purpose. The copper sheet, varnished on the lower side, is attached in many places to the lead lining of the trough by soldered slips of copper, the solderings being thoroughly coated with varnish or marine glue. This copper sheet forms the copper plate of the Daniell's cell.

The zinc of the cell consists of four of an unusually large size (56 centims., 22 inches square) of the gridiron zincs used in Sir William Thomson's well-known tray Daniell, which is largely employed at most of the submarine telegraph stations in connexion with the Siphon Recorder, and is a modification of the constant Daniell's battery described "Proc. Roy. Soc.," 1871, p. 253. These gridiron zincs rest on small blocks of wood at the four corners of each, and they are connected together by strong copper slips at the corners, and by means of two diagonals of thick copper rod which are soldered at the outlying corners and at the four meeting corners in the middle of the square.

FIG 3.



Very powerful pairs of electrodes are soldered on at each of the four corners of the cell, though only one of these pairs is shown in the diagram at D. These electrodes are formed of flexible copper

rope, terminated with two thick slips of copper which are fastened together mechanically, but electrically insulated, and form the flat duck's-bill shaped piece *ll*, shown in section, fig. 3. The electrodes of any instrument which is to be connected with the cell may be also terminated with two copper slips, which are pushed on *over* the piece *ll* and held on by india-rubber bands.

The mode of supplying the cell with sulphate of copper still remains to be described, and it is this which constitutes its most important peculiarity. The liquid contained ordinarily in the tray Daniells is somewhat dilute solution of sulphate of zinc (sp. gr. 1·12 to 1·14), the lowest layer of which is saturated with sulphate of copper. The sulphate of zinc solution is kept at the proper density by occasionally drawing off quantities of strong liquor and replacing by pure water, testing by a hydrometer or with specific gravity beads. The sulphate of copper is supplied by dropping a proper quantity of blue vitriol in crystals into the cell round the edges. This is sufficiently easy when the cells are of moderate dimensions, and where a large number of them is being constantly used and regularly attended to in a business-like way, as at a telegraph station. When, however, the size of the tray is very large, and when the cell is being used intermittently, this simple method becomes difficult and unsatisfactory; and, owing to want of uniformity of the solutions, the cell is rarely obtained in such a condition as to be ready to give the results which ought to be expected from it. The copper sulphate supplied in crystals does not dissolve sufficiently quickly and spread itself over the whole surface of the lower plate, and difficulties arise as to the dilution of the sulphate of zinc without stirring up from the bottom remnants of unused sulphate of copper.

To avoid these troubles, and to make the starting and stopping of the action of the cell easy and rapid, I have arranged to supply the sulphate of copper in the state of solution, and to deliver and to remove it very gently but very quickly, and also simultaneously, at many parts of the bottom of the cell.

X and Y are two earthenware tubs placed one on either side of the cell; and it should here be remarked that the cell is raised up from the floor on common building bricks. From these vessels a number of siphons or delivering tubes, *t*, *t*, *t*, of which ten* are shown in the diagram, proceed to different parts of the cell. The tubes are of thin glass, except the bend *ss*, which is of flexible india-rubber tubing, and enables the extremities to be taken with ease out of the vessels X and Y for clearance or for starting of the siphons. The tubes are held in position by passing through holes in the boards *w*, *w*, and the glass portions are bent in a gas flame to the proper shape for

* I have recently doubled the number with great advantage.

delivering to the various parts of the bottom of the cell. The vessels X and Y are furnished each of them with a shelf of thin sheet copper perforated with many holes, the level of the shelf being considerably above the level of the extremities of the siphon tubes. On these shelves a large quantity of sulphate of copper in crystals is placed.

To raise the vessels X and Y up, and cause the siphons to run, two boards are provided which are pushed in beneath the vessels. One of these, *mm*, is shown beneath the vessel X in the figures. The boards are made of exactly the proper thickness to cause a layer of chosen depth to be run into the cell out of the vessels, or out of the cell into the vessels, according as the vessels are raised up on the boards or are lowered down when the boards are removed. The thickness of the boards depends, of course, on the relative sectional areas of the two vessels and of the cell. The depth of the layer which is spread over the bottom of the cell should be not more than one-eighth of an inch. The bottom of the cell must be very carefully levelled in order that the layer of charged liquor may be uniformly distributed, and that the half-spent liquor may be uniformly drawn off.

In setting up the cell in the first instance I commence with the cell charged with weak solution of sulphate of zinc (sp. gr. 1·12), while the vessels X and Y are charged with nearly saturated sulphate of zinc solution. This liquor is still able to take up a very large quantity of sulphate of copper; and water saturated with both sulphate of zinc and sulphate of copper is much more dense than saturated solution of either salt alone. When the cell is to be started the two reservoirs are raised into position. The thin layer of dense liquor spreads itself over the bottom in a few minutes, and the cell is ready for use. When the use of the cell is to cease the reservoirs are lowered down, and nearly the whole of the dense liquor runs back into them. This should always be done at night, or during any considerable time when the cell is to be out of use.

During the action of the cell sulphate of zinc is constantly being formed, and it is necessary frequently to run off a portion of the contents of the cell and fill up with pure water. I have planned for a system of tubes (arranged similarly to those just described) for siphoning off into independent vessels, and with a cell of very great dimensions, such as I am now constructing, they will probably be necessary. With the cell which I have been using for nine months, however, I have been able to dispense with them.

It only remains for me to give the dimensions of the cell now in use, and of a new one in process of construction. The tray of the former is 47 inches square inside measurement, and $3\frac{3}{4}$ inches deep. The zincs are each 22 inches square, and are raised up $\frac{3}{4}$ inch from

the bottom. The copper sheet is about 0·2 of a millim., or 0·008 inch thick. It is probable that it would be better a little thicker. With this cell I can command about 45 amperes for hours together at any time, only requiring to take, now and then, a fresh supply of sulphate of copper from the reservoirs. The new cell which is in process of construction is 69 inches long by 46 inches, and $3\frac{3}{4}$ inches deep. It has four reservoirs, and six zincs placed in two rows of three each.

Received June 19.

P.S.—In the foregoing paper, I have described the construction and capability of a cell, whose dimensions are given, constructed about the beginning of July, 1883. It was found necessary, almost whilst I was writing, to take the cell to pieces, as it had become blocked up with copper deposited in the natural course of nine months' working. In putting the cell together again, with attention to *small particulars*, which I shall not describe as they will naturally occur to anyone who desires to use the arrangement, but which, I need scarcely remark, make, with such great currents, all the difference between success and non-success, I have obtained, through a current galvanometer of which the resistance is $\frac{1}{500}$ of an ohm, a current of 63 amperes, which was quite steady.

J. T. BOTTOMLEY.

June 16th, 1884.

XVI. "On the Permanent Temperature of Conductors through which an Electric Current is passing, and on Surface Conductivity, or Emissivity." By J. T. BOTTOMLEY. With a Note by Sir WILLIAM THOMSON, F.R.S. Received June 17, 1884.

(Preliminary paper.)

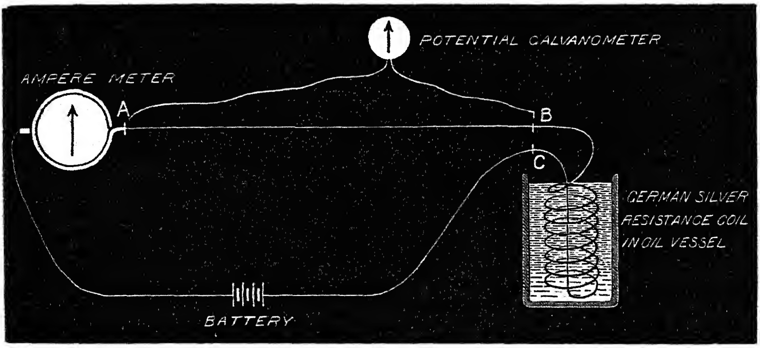
The experiments of which an account is given in the following paper were commenced several months ago in connexion with a theoretical investigation by Sir William Thomson, "On the Effect of Clothing," showing the dependence of the effect on the relation between the dimensions of the covered body, and the dimensions and physical properties of the covering. A primary object of the experiments was the determination, for practical purposes, of the temperature of electric light conductors. The preliminary experiments have led to results which seem to be of considerable importance, and they point to the necessity for a fresh determination of the "emissivity"

or "surface-conductivity" of bodies of various dimensions, and under various circumstances as to surroundings and as to air pressure.

With respect to the determination of the permanent temperatures of wires carrying electric currents, very little seems to have been done experimentally. In fact, the only important experiments on the subject seem to be those of Mr. Preece, on the currents required to fuse and to raise to a dull red heat platinum wires of different diameters. These were communicated to the Royal Society in a paper read April 3, 1884. I may here remark that the results given by Mr. Preece differ in a very definite way from those which I have obtained; but it is to be noticed that the temperatures of Mr. Preece's determinations were much higher than those at which I have, up to the present, experimented.

The method which I have employed for determining the heating effect of a given current in a given wire has been to find the *resistance* of the wire, first with a very feeble current passing through it, and afterwards with the given more powerful current passing; and after it has been passing for a sufficiently long time for the wire to have acquired a temperature permanent under the given circumstances. The resistance of the wire becomes increased as the temperature rises, and from the increase of resistance the increase of temperature can be calculated.

FIG. 1.



In doing this I have used two methods of experimenting. In the first method the wire under experiment was connected in series with the battery supplying the current, an ampere-meter, and a standard resistance of thick German silver wire. The German silver wire was immersed in heavy paraffin oil to keep it cool, and the wire under experiment was left freely exposed to the air, but carefully guarded by paper screens placed at safe distances, both from draughts and

currents of air, and from radiation of the sun or laboratory fire, and from the persons of the observers.

To commence the experiment a very feeble current was passed through the line consisting of the copper and the German silver wire, and by means of a sensitive reflecting galvanometer of high resistance the differences of potentials at the two ends of AB, the copper wire, and of BC, the German silver wire, were determined (fig. 1). The ratio between these differences of potentials is the same as the ratio between the resistances of AB and BC. Then, BC, the standard of German silver, being known, the resistance of AB was calculated. This experiment, therefore, gave the resistance of AB *cold*.

A much more powerful current was then caused to flow through the line, and was kept up for a sufficient time until AB had taken a permanent temperature. The terminals of the potential galvanometer were again applied, and the differences of potentials at the extremities of AB and of BC once more determined, and thus the ratio of their resistances. If the resistance of BC had remained absolutely constant, the resistance of AB *hot* from the effect of a current, the magnitude of which is shown by the ampere-meter, would now be known. This was practically the case. The quantity of oil surrounding the German silver wire was so considerable that the temperature rose but little, and the change of resistance of German silver with temperature is so small that it was unnecessary to apply any correction. It would be easy to keep the temperature of the oil quite constant by occasionally passing a test tube containing a little ice or a small freezing mixture from place to place through it, and then stirring the whole up thoroughly.

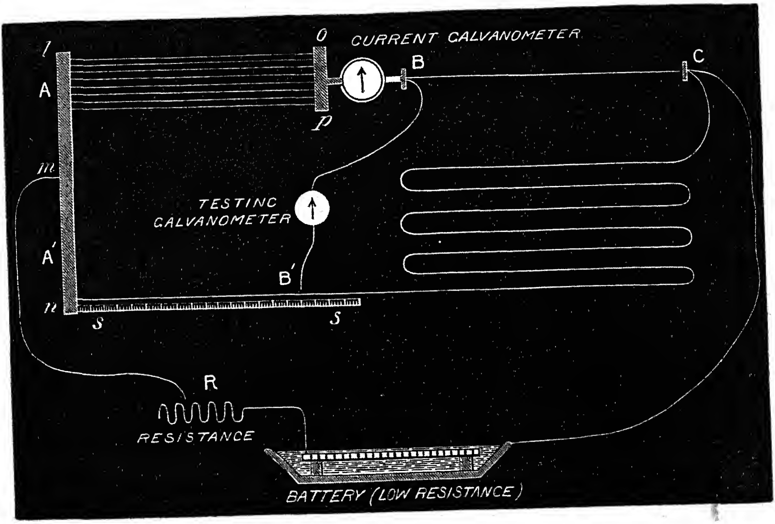
The resistance of the copper wire first *cold* and afterwards *hot* having been determined, and the temperature of the wire when cold being known (as at the beginning the temperature of the wire was the same as that of the air), it was easy to find the temperature of the wire hot by applying the formula for temperature correction for the resistance of copper.

I have also used, latterly, for passing a powerful current through a wire under experiment, and for determining the resistance of the wire while the current is flowing, a Wheatstone's bridge, suitably arranged for the purpose, in accordance with a plan suggested by Sir William Thomson, and I find this method more sensitive and more generally convenient than the potential method.

The diagram (fig. 2) explains the arrangement. ABC and A′B′C are the two branches of the Wheatstone's bridge, and the four conductors are made up as follows: BC is the wire under experiment as to heating by the current; *lmn* and *op* are two bars of very stout copper, and between *op* and the portion *lm* of the bar *lmn* there are soldered a considerable number of stout copper wires making up a

compound conductor of small resistance, and having very large surface exposed to the air for cooling. Between *op* and B a current galvanometer of very small resistance is introduced for measuring the whole current flowing in the branch ABC, and therefore through the

FIG. 2.



experimental wire BC. The branch A′B′C is a single long wire having many times the resistance of the branch ABC, and many times the resistance of the battery used, and thus only a small portion of the current passes through it—a quantity, in fact, unable to heat this branch to any appreciable extent. The testing galvanometer is introduced between B and B′, the contact at B′ being shifted about till a "balance" is obtained; and from the position of B′ on the scale *ss* the ratio can be calculated between A′B′ and B′C, when the equipotential point has thus been found.

In experimenting, the wire BC is soldered between stout copper slips for the purpose, shown in the diagram at its extremities, and its resistance is taken with a very small current passing through it; the resistance R being provided for the purpose of varying the current. The current is then increased by diminishing the resistance R. When this is done, the wire BC becomes heated, and its resistance increases; but the conductor AB and the branch A′B′C remain unaltered, though the ratio between the parts of the latter corresponding to the "balance" is changed.

Let c be the resistance of the conductor AB; let r_1 and r_2 be the

resistances of BC when, respectively, the weak current and the stronger current are applied; also let a_1, a_2 and b_1, b_2 be the corresponding respective lengths of the shorter and longer parts of the branch A′B′C.

Then

(1) $$c : r_1 :: a_1 : b_1$$

and (2) $$c : r_2 :: a_2 : b_2$$

Hence $$\frac{r_2}{r_1} = \frac{a_1 b_2}{a_2 b_1}$$

Let t be the temperature of the air of the room where the experiment is made, and let it be assumed that the temperature of the wire BC is the same as that of the air during the first experiment with the weak current; let θ be the temperature of the wire when the stronger current is passing. Let σ_t and σ_θ be the specific resistance of these substances at these temperatures.

Then $$\frac{r_2}{r_1} = \frac{\sigma_\theta}{\sigma_t}$$

the change in dimensions of the wire due to heating being left out of account. The values of σ_t and σ_θ can be found, as has been remarked already, for copper and for several other substances from well-known tables and experiments.

My first experiments, after some preliminary trials had been made, were on a series of copper wires, all of them cut from the same piece, and of the same length, but some of them bare and others covered with different insulating coatings. The wires were about No. 21 of the old B.W.G., 0·81 millim. in diameter and half a metre in length. The resistance of each, with only as much current passing as sufficed for the ordinary application of Wheatstone's bridge, was 0·0183 B.A. unit. These were tested by the potential method described above. Eight wires were taken, and their surfaces were prepared as follows:—

A. Surface amalgamated with tin and mercury.
B. Surface amalgamated with mercury by rubbing on mercury nitrate and washing.
C. Bare surface (dim).
D. Covered with cotton and shell-lac.
E. Cotton-covered.
F. Shell-lac varnish.
G. Bare, polished with fine emery.
H. Thick coating of lampblack.

These wires were tested successively with currents of 1·2 amperes,

3 amperes, 5 amperes from the battery; then with 10 amperes, 20 amperes, and about 30 amperes from the dynamo-electric machine; the last-named current overheating both the coverings and the wires and burning some of them up. The differences of temperature were wonderfully small, but they were decided. The following table shows the wires arranged in order of coolness, and therefore of the efficiency for emission of heat of their respective coverings :—

Current—

1·2 amperes	H	D	C	E	F	G	B	A
3 „	H	D	C	E	F	G	B	A
5 „	H	D	C	E	F	G	A	B
5 „	H	D	C	E	F	G	A	B
10 „	H	D	E	C	F	B	G	A
20 „	D*	F	H	E	C	G	B	A

With about 30 amperes—

Tin and mercury	A	Surface slightly fused and blistered.
Mercury	B	Wire totally dissipated.
Bare surface (dim) ..	C	No visible alteration.
Cotton and shell-lac ..	D	Coatings fused together.
Cotton covering......	E	Covering burned off.
Shell-lac varnish	F	Covering fused.
Bare polished	G	No visible alteration.
Lampblack	H	No visible alteration.

It appears therefore that the surfaces stand as follows in order of cooling efficiency :—

H, lampblack.
D, cotton and shell-lac.
C, bare wire, dim,—in common unaltered condition.
E, cotton-covered.
F, shell-lac varnish.
G, polished copper.
B, mercury surface.
A, tin and mercury.

In order to show a comparison as to the amount of cooling effect, I may here quote the temperatures of these wires with a current of 5 amperes passing, and in air at 11° C. This was the highest current I was able to apply with the battery I then had at command, and with the mode of experimenting which I was using at the time these experiments were made. The temperatures were determined in all the cases mentioned in the table above, but with the higher

* Covering probably half burned in D and F. The wires bracketted were equally heated.

currents there was in this series of experiments some uncertainty owing to the fluctuations of the potential of the dynamo-electric machine; and, moreover, it is the less necessary for me to quote these here as I hope shortly to make the whole investigation more thorough and more satisfactory.

Temperature of wires with 5 amperes passing, the temperature of the air being 11° C.—

A	Tin and mercury..............	22°·5
B	Mercury	22 ·5
C	Bare wire (dim)	21 ·75
D	Cotton and shell-lac	21 ·5
E	Cotton-covered.................	21 ·75
F	Shell-lac varnish..............	21 ·8
G	Polished copper	22 ·0
H	Lampblack coating............	21·0

The numbers in this table show that the permanent temperatures assumed in the various cases differ wonderfully little with so great differences as to clothing; and that, at the least, there is no tendency in the covered wires to attain a temperature higher than that taken by the uncovered wires. With stronger currents it is made much more apparent that the covering does, on the contrary, favour loss of heat by emission at the sides with a wire of the size here referred to.

Specifying number B.W.G. and nature of covering.	Length of wire. Centims.	Resistance of 100 centims. B.A. units.	Diameter of wire in millims.	Diameter of covered wire, outside measurement.	Current (amperes).	$t-\theta$	Emissivity.
No. 22, silk-covered	100	·0395	·76	·96	10	23°·4	0·001333
No. 26, cotton-covered ..	100	·094	·50	·88	10	58	0·001385
No. 26, silk-covered	100	·1115	·45	·57	9·8	70	0·002020
No. 22, gutta percha ...	100	·0455	·72	1·67	10	24	0·000854
No. 22, tinned, gutta percha-covered, and double cotton covered outside	100	·0432	·73	1·86	10	23	0·000759

The foregoing table contains the results of experiments on five copper wires of good conductivity with various coatings. In the first

five columns are given particulars regarding the wires and the current passing through them. The sixth column shows the difference $t-\theta$ between the temperature assumed by the wire and that of the surrounding air, the latter being in all these cases about 12° C. In the seventh column is given the emissivity of the outer surface,—or the quantity of heat (C.G.S. units) lost by the surface per second per square centimetre of cooling surface per degree Centigrade of difference of temperature of cooling surface and surroundings.

I may next refer to experiments on a length of wire which was first tested in its ordinary state, then when highly polished, and afterwards with the surface covered with coatings of shell-lac varnish thickly put on and gradually increasing.

Here I had—

Length of copper wire	50 centims.
Diameter..............	0·97 millim.
Resistance with feeble current............	·01268 B.A.U.
" " German silver wire	·020
Ratio...	·634

With current of 10 amperes I found—

Condition of surface.	Ratio of copper to G.S.
Bare unpolished	·6635
Bare highly polished	·6534
One coating shell-lac	·6547
Two " "	·6510
Three " "	·6466
Three coatings shell-lac and a covering of cotton wool after three minutes ..	·6595
Ditto six minutes....................	·6668
Ditto nine "	·6325

Another wire was similarly treated with coatings of Brunswick black and thin tissue paper, and gave the following results:—

	Ratio of copper to G.S.
Bare wire	·9895
One coating Brunswick black............	·9745
One B.B. 5-ply fine paper	·9833
One B.B. 5 paper 1 B.B.................	·9844
One B.B. 5 paper 2 B.B.................	·9853
Same with 20-ply paper added	·9982

The current was in every case 10 amperes. Increase in the numbers indicates increase in the resistance of the tested wire, and therefore increased temperature; while decrease in the numbers quoted shows decrease in the temperature. The change in the permanent temperature is not more than two degrees; and the result of these experiments may be briefly stated to be this, that there was exceedingly little effect in the way of keeping up the temperature of a No. 20 B.W.G. copper wire, about 1 millim. in diameter, by covering it up with paper and Brunswick black till it attains an external diameter of more than a centimetre and a half.

A large number of experiments were tried of a kind similar to the one just quoted, and all of them giving similar results for wires of similar dimensions. I propose with improved arrangements which I have now at command, to obtain numbers which shall be more accurate than those which I have yet obtained; and to extend the experiments to the case of wires of larger diameters.

I have also made some preliminary experiments on copper wires of various diameters without solid covering, cooling in air and in vacuum. In commencing these experiments I used as a formula for controlling my results an expression derived as follows:—

A current passing through a given wire produces heat of which the amount, according to the well-known formula of Joule, is given by—

$$H = C^2R/J \quad \dots\dots\dots\dots \quad (1),$$

where C is the current, R the resistance, J Joule's equivalent, and H the quantity of heat produced per second, each being reckoned in C.G.S. units. Let l be the length of the wire experimented on, and d its diameter; and let σ_t be the specific resistance of the substance at the temperature t°. Then—

$$R = \frac{\sigma_t \times l}{\frac{1}{4}\pi d^2} = \frac{4\sigma_t l}{\pi d^2}$$

Hence from (1)

$$H = \frac{C^2}{J} \times \frac{4\sigma_t l}{\pi d^2} \quad \dots\dots\dots\dots \quad (2).$$

Now let H′ be the quantity of heat lost by the wire by emission from the surface; let e be the emissivity; and let θ be the temperature of the surroundings within which the wire is cooling.

Then—

$$H' = \pi dl \cdot e(t-\theta) \quad \dots\dots\dots\dots \quad (3).$$

But when a permanent temperature is attained H must equal H′, and hence we have from (2) and (3),

$$e = \frac{4C^2\sigma_t}{J\pi^2 d^3(t-\theta)} \quad \dots\dots\dots\dots \quad (4).$$

Experiments have been made by Mr. D. Macfarlane ("Proc. Roy. Soc.," 1872, p. 93) for the purpose of determining the emissivity of surfaces in absolute measure. Mr. J. P. Nichol has also obtained results, which were communicated to the Royal Society of Edinburgh by Professor Tait, 1869–70; and which have been reduced to absolute measure by Professor Everett, "Units and Physical Constants," chap. ix, § 137.

On calculating the value of the emissivity for small copper wires by means of the formula (4) given above, I have found in every case a much greater emissivity than was obtained by these experimenters; and so far as I have been able to go up to the present the emissivity seems to increase as the diameters of the wires experimented on are diminished. Macfarlane's determinations of emissivity were made for a copper globe of 4 centims. diameter, and found to be about $\frac{1}{4000}$ of the thermal unit C.G.S. per square centim. per second per degree of difference of temperature for a polished surface with a little over 60° of excess of temperature, and for a blackened surface with excess of 5° or under. For round wires of small diameter I have found very much larger emissivity than $\frac{1}{4000}$. I have obtained different values of e for wires of different sizes, varying from $\frac{1}{2000}$ with a polished wire diameter 0·83 millim., and excess of temperature 27°·5 C., down to $\frac{1}{400}$ with a wire of 0·40 millim., and excess of temperature 24° C.

In order to examine the question more thoroughly, I have commenced experiments in which I am endeavouring to do away the part of the emissivity which is due to convection and carriage of the heat by air. I have experimented to a certain extent on small wires in the nearly perfect vacuum obtainable with the mercurial pump, and I am preparing for a more complete series of experiments. It seems certain that this method of experimenting will give a very accurate way of determining the value of e in absolute measure for the surfaces of wires.

During the writing of this paper Professor Stokes has kindly reminded me of the experiments made by Mr. Crookes ("Proc. Roy. Soc.," vol. 31, p. 239). I find that my results, so far as they go, agree perfectly with those of Mr. Crookes, showing a decrease in the emissivity due to lowering the air pressure, this decrease being very small for a reduction down to one-half or one-third of the ordinary atmospheric pressure, but becoming very great with the almost perfect vacuum obtained with the mercurial pump.

The following table shows the emissivity of a copper wire with bright surface half a metre long, 0·40 millim. in diameter, and sealed into a glass tube, about 1·5 centim. internal diameter:—

Current, Amperes.	Pressure, 760 millims.		Pressure, 380 millims.		Pressure, 180 millims.		Very high vacuum. Pump worked continuously.	
	$t-\theta$	e	$t-\theta$	e	$t-\theta$	e	$t-\theta$	e
1	$4^\circ\cdot7$	$\frac{1}{1822}$	$4^\circ\cdot5$	$\frac{1}{1784}$	$5^\circ\cdot5$	$\frac{1}{2176}$	17°	$\frac{1}{6443}$
2	22·5	$\frac{1}{2084}$	21·5	$\frac{1}{1996}$	23·5	$\frac{1}{2174}$	68	$\frac{1}{5620}$
3	56	$\frac{1}{2114}$	58	$\frac{1}{2180}$	55	$\frac{1}{2082}$	140 ?	$\frac{1}{4606}$*

In the somewhat crude state of my preliminary experiments I have not considered it necessary in calculating to make any allowance for heat lost at the ends of my wires by conduction to the masses of metal to which they were soldered. The following appended note, however, contains an investigation of the correction which, in future experiments, it will be necessary to apply.

Note by Sir W. Thomson.

To estimate effect of conduction of heat from the tested wire through its ends: suppose the ends to be kept at the atmospheric temperature. This supposition corresponds to the greatest possible degree of the effect in question. (Adopting Fourier's notation, in the first four symbols), let v be the excess of temperature in the wire, at distance x from one end:

a, the length of the wire;
k, its thermal conductivity;
h, the thermal emissivity of its surface;
g, the girth of its cross section;
A, the area of its cross section;
s, the specific electric resistance of its substance (C.G.S.);
$\frac{1}{10}$ Ac, the strength of the current (C.G.S.);
(or c, the current in amperes per square centimetre of cross-section);
and J, Joule's equivalent multiplied by the force of gravity at Manchester ($42344 \times 981\cdot34 = 41\cdot55$ centimetre-dynes), the dynamical equivalent of the thermal unit (C.G.S.),

we have by Fourier and Joule:

* Temperature probably much too low. The wire, sagging down, touched the glass tube in several points.

$$\frac{d}{dx}\left(k\mathrm{A}\frac{dv}{dx}\right)-ghv+\frac{c^2s}{100\mathrm{J}}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

Neglect now the variations of k and s with temperature, and suppose the conductor of uniform material and area and girth of cross-section throughout its length from $x=0$ to $x=a$. Equation (1) becomes—

$$\frac{d^2v}{dx^2}-\frac{gh}{k\mathrm{A}}v+\frac{c^2s}{100\mathrm{J}k\mathrm{A}}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

and its integral, fulfilling the end conditions, is—

$$v=\frac{c^2s}{100\mathrm{J}gh}\left[1-\frac{\epsilon^{-mx}+\epsilon^{-m(a-x)}}{1+\epsilon^{-ma}}\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

where $$m=\sqrt{\frac{gh}{k\mathrm{A}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8).$$

For round wire or rod of diameter D, we have—

$$g=\pi\mathrm{D}, \text{ and } \mathrm{A}=\tfrac{1}{4}\pi\mathrm{D}^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

whence $$m=\sqrt{\frac{4h}{k\mathrm{D}}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10).$$

For copper we have $k=\cdot 91$ (Ångström).

Now put $$h=\frac{1}{n\times 1000}$$

where n might be about 4 for the ranges of temperature in Mr. Bottomley's experiments, if we could judge from Macfarlane's experiments on the cooling of a globe of copper of 4 centims. diameter; and in the wires experimented on by Mr. Bottomley, $\mathrm{D}=\cdot 08$. Hence for these wires—

$$m=\sqrt{\frac{4/n}{\cdot 91 \,.\, 80}}=\frac{1}{8\cdot 5}\sqrt{4/n} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

Hence if $n=4$, $m=\frac{1}{8\cdot 5}$. But Mr. Bottomley's experiments show n to be more nearly $=1$, and to be actually <1 for wires of somewhat less diameter than $\cdot 08$. Hence we have, as a practical rough approximation, $m=\frac{1}{4}$; and (7) becomes—

$$v=\frac{c^2s}{100\pi\mathrm{D}h\mathrm{J}}\left[1-\frac{\epsilon^{-\frac{x}{4}}+\epsilon^{-\frac{a-x}{4}}}{1+\epsilon^{-\frac{a}{4}}}\right] \quad . \quad . \quad . \quad . \quad . \quad . \quad (12).$$

In Mr. Bottomley's experiments $a=50$, and therefore

$$\epsilon^{-\frac{a}{4}}=1/\epsilon^{-12\cdot 5}=1/268000,$$

which may be neglected; and we have—

$$v=\frac{c^2s}{100\pi\mathrm{D}h\mathrm{J}}\left[1-\epsilon^{-\frac{x}{4}}-\epsilon^{-\frac{50-x}{4}}\right] \quad . \quad . \quad . \quad . \quad . \quad (13),$$

which shows that at a distance of 4 centims. from either end, the temperature is less than in the middle by ϵ^{-1}, that is 1/2·7 of the middle temperature; at 8 centims. it is less by 1/7·4; and at 12 centims. it is less by 1/20. Thus we see that the cooling by the ends is very sensible through a quarter of the length from either end; and must be carefully allowed for by aid of (7), unless lengths of considerably more than half a metre are taken. But we also see that the results stated in this preliminary paper are not sensibly affected, or hardly sensibly affected, by cooling from the ends.

XVII. "The Theory of Continuous Calculating Machines, and of a Mechanism on a New Principle for this and other purposes." By H. S. HELE SHAW. Communicated by Sir WILLIAM THOMSON. Received June 19, 1884.

(Abstract.)

The paper commences with a statement of the conditions which must be fulfilled by the mechanism of a continuous calculating machine of the most general kind. It is shown that both the operations of differentiation and integration must be performed by such an instrument.

The only hitherto known mechanisms with which it appears possible to accomplish this are the "disk and roller," and its modification the disk-globe and cylinder-integrator, or with some device which relies on the same principle of action. A brief account is given of the applications of the disk and roller itself, which mechanism was first suggested by Poncelet, for integrating the products of the two variables in a traction ergometer, and has since been applied by various inventors—the disk being sometimes replaced by a cone, as a planimeter or platometer and integrator.

The applications for the converse process of differentiation are less known, and do not appear to have been brought into successful operation.

A speed indicator, in which a screw replaced the usual axis of the roller, the roller forming the nut, was made three years ago by the author. In this case the screw was driven at a speed which varied with the space passed over by the moving body, and the disk at a constant speed by a clock. The position of the roller on the disk was

then a measure of the ratio of the increase of space to that of time, and in the limit is a measure of their differential coefficient, that is (since $v=\frac{ds}{dt}$) a direct measure of the velocity of the moving body. The author afterwards found that this idea had been suggested some time previously by a correspondent in "Engineering." Quite recently (May 24th) a speed indicator on this principle, but with a cone instead of a disk, was exhibited before the Physical Society, and at the same meeting the Secretary himself showed the disk and roller speed indicator which he had re-invented.

The disk and roller has, however, two kinds of defects, which account for its practical failure for the foregoing purpose, and render its accuracy as an integrator almost a matter of impossibility; though in the latter case the error for reasons which are explained is more difficult to detect. These kinds of defects are:—

(1.) Those which are the mechanical result of the principle of action itself;

(2.) The limited range of action of the instrument.

(1.) The very conditions under which the mechanism acts are contradictory, and are shown to lead to three special defects:—

(i.) Grinding action between the edge of the roller and the face of the disk.

(ii.) Necessity for the application of force, in order to change the position of the roller.

(iii.) Error in numerical results.

These defects are considered, and a modification of the disk-globe and cylinder-indicator of Professor James Thomson, which was designed to obviate them, is suggested for the converse process.

(2.) The second kind of defect led the author to modify the disk and roller, so that a range of action, in theory infinitely great, was obtained. This is not so easy to use, the graduation of the scale having to be made according to an equation $y=\frac{x}{R-x}$, instead of simply $y=Rx$, as in the first form; but it was new, and led to the fact being made clear that there was a close analogy between these two forms and two forms of what may be called the "sphere and roller mechanism," which with certain results were communicated to the Physical Section of the Bristol Naturalists' Society in November last.

It was the endeavour to overcome the frictional defects of the disk and roller which still remain in this form of mechanism, and to render the application of the sphere and roller possible for practical purposes, that led to the investigation which forms the real subject of the paper.

The result has been a mechanism which consists of two distinct features:—

(1.) A mechanical principle, by which an arrangement of spheres and rollers is made capable of transmitting motion, which can be independently varied in any required manner.

(2.) The arrangement by which mathematical results can be at once obtained with any required numerical quantities, however large.

The rest of the paper consists in a discussion of these principles, and of the practical applications which, beside enabling the operations already mentioned to be performed, enable logarithms and roots to be calculated, besides giving a convenient computator for reducing tables of results.

The almost entire absence of friction enables a chain of spheres and rollers to be employed in a very compact form and with accurate result. Thus, with two sets, the moment of an area, the integration of volumes, &c., can be obtained; with three, the moment of inertia, and other results. In fact, the operation

$$\int F_1(x) . F_2(x) \quad . \quad . \quad . \quad F_n(x)dx$$

can be performed with n sets of the fundamental form of mechanism.

By using the screw arrangement, the value of a ratio or differential coefficient can be obtained; and, by using two sets, the second differential coefficient, or

$$\frac{d^2s}{dt^2}=\text{acceleration},$$

or rate of change of velocity, is given. By using the integrating and differentiating mechanism in conjunction, *rate of working*, or H. P. can be indicated on a dial.

Various practical applications, such as a planimeter, a sliding rule for giving at once A, M, and I, a H. P. indicator are shown. Finally, the application of the mechanism to the transmission of power is considered, as well as the mechanics of problem, and a modification of an important nature for the roller bearings is described and illustrated.

XVIII. "On the Experimental Determination of the Index of Refraction of Liquefied Gases." By Dr. L. BLEEKRODE. Communicated by Dr. GLADSTONE, F.R.S. Received June 19, 1884.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 20th.

Presents, May 1, 1884.

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"The Variation of Stability with Draught of Water in Ships." By F. Elgar, Professor of Naval Architecture in the University of Glasgow. Communicated by Professor Sir William Thomson, F.R.S. Received March 6, 1884, Read March 13.

Of all the properties possessed by a ship none is more vital to her safety and efficiency than that of stability. At the same time none is dependent for its existence and amount upon so many, or such diverse and variable, circumstances as it. The stability of a ship is regulated and determined by her outward size and shape, and also by the separate amounts and positions of all the weights that go to make up her structure, equipment, and loading. No change of any kind can be made in dimensions or form, or in the quantities or distribution of the various items of equipment, stores, or cargo without affecting stability. It is, of course, essential to the safety of every vessel that her stability should not become reduced during all the changing conditions of her employment and career below a certain definite amount. The result of neglect in this respect may be a dangerous inclination or complete capsize when unlooked for, or exceptionally trying, emergencies occur. Deficient stability, whether caused by faulty design or stowage, may admit of a vessel being suddenly capsized by the action of the wind and waves, or of her being forcibly heeled to a dangerous angle of inclination by the shifting of some of her internal weights, such as coals or cargo. Although in every vessel there is a minimum limit below which it is not prudent or safe to diminish the stability, it does not follow that this limit is the same or similar in character in all sizes and types of ships.

It is not only necessary to guard against the stability of a ship becoming reduced below a safe minimum amount, but there is also a maximum limit which it should not be allowed to exceed. Excessive

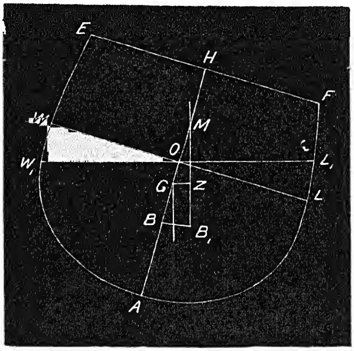
stability has its own peculiar objections and dangers. It causes heavy and uneasy rolling which may not only be uncomfortable and trying to passengers and crew, but at times sufficient to strain and even fracture portions of the structure, and displace or damage some of the fittings. The masts and rigging generally suffer most on account of their distance from the axis of rotation, and the change of motion in them at the end of each roll, being greater than that of any other part of the vessel; and also because their attachment to the hull is less direct and secure. The straining effect of too great stability often shows itself early, and in a marked manner, upon the masts and rigging of a vessel. Seamen frequently say of a ship that is laden with heavy cargo placed low down in the hold, that unless some of the weights are raised, *i.e.*, unless the stability is reduced, she will roll her masts out. The violent and deep rolling caused by excessive stability also tends to move over towards one side of the ship such cargoes as grain or coals which are free, to some extent, to shift as a whole; or such portions of other classes of cargo as admit of being displaced. Speaking generally, it may be said that, whereas large stability helps to prevent inclination to a great angle in the event of cargo shifting, the very possession of such large stability often increases the chances of shifting.

The stability of a ship at a given draught of water, and with a specific description of loading can readily be calculated, and it has become usual to make such calculations at the Admiralty and in a few of our leading mercantile shipyards. The practice is to construct a curve of metacentres, which shows how the height of the metacentre varies with draught of water, and is affected by such changes in the external bulk and form of the immersed portion of the hull as may be caused by increasing or diminishing the draught of water. A curve of stability is also constructed for one or more fixed draughts of water, and for certain intended, or estimated, weights and distributions of load. These curves show what the righting moments for such draughts of water and descriptions of loading amount to at successive angles of inclination from the upright up to 90°, or to the angle at which the stability vanishes.

The curve of metacentres is obtained by calculating the height of the metacentre at several fixed draughts of water, setting up those heights as ordinates of a curve whose abscissæ are proportional to the corresponding draughts of water, and drawing a fair curve through the points thus obtained. The term "metacentre" was originated by Bouguer, who published the first investigations into the subject in his "Traité du Navire," which appeared in 1746. The metacentre of a floating body is the point of intersection of a vertical line through the centre of gravity of the volume of displacement when the body is inclined through an indefinitely small angle from a

position of equilibrium, with the vertical through the centre of gravity of the displaced volume in the original position of equilibrium.*

FIG. 1.



For instance, if AEF in fig. 1 represent the transverse section of a ship upon which WL is the intersection of the water-line plane when the vessel is upright and in a position of equilibrium, and B the position of the centre of gravity of the volume of displacement WAL—or centre of buoyancy as it is commonly called; and if AH be the vertical through B when the vessel is floating in equilibrium at the water-line WL, then if the vessel be inclined through a small angle WOW_1, and the point B_1 represent the centre of buoyancy of the immersed volume W_1AL_1, the vertical B_1M, drawn through B_1, will intersect the original vertical AH in a point M. M is the metacentre when the angle of inclination WOW_1 is indefinitely small. It then represents the ultimate intersection of the new vertical through B_1 with the original vertical BH. In a ship the vertical corresponding to the upright position of equilibrium is, for all practical purposes, the intersection AH of the middle line longitudinal plane with the transverse section.

Bouguer showed that the position of the metacentre limits the height to which the centre of gravity of a floating body may be raised without making it unstable, and that the righting moments at small angles of inclination from a position of stable equilibrium are proportional to the height of the metacentre above the centre of gravity. This is readily seen, because if G in fig. 1 be the position of the centre of gravity of a ship, and GZ the horizontal distance between

* It is a moot question whether the term metacentre should not be made to embrace all the ultimate intersections of consecutive verticals through the centres of gravity of the displaced volume. Bouguer calls the locus of such intersections the *metacentrique*.

G and the vertical through the centre of buoyancy B_1, then $W \times GZ$ is the righting moment; W being the weight of the ship. But $GZ = MG \sin WOW_1$; and therefore when WOW_1 is indefinitely small, the righting moment is proportional to $W \times MG$. While G remains below M the moment is always a righting one, and tends to restore the ship to the upright position, which in this case is one of stable equilibrium; but if it be above M the tendency is to move the ship farther away from the upright till an inclined position of stable equilibrium be reached, or to capsize her. The curve of metacentres for a ship, which gives the height of the metacentre at all draughts of water, indicates therefore the limit above which the centre of gravity cannot be raised by changes in the amounts or positions of any of the weights without causing her to become unstable. Sufficient stability for practical working requirements and for purposes of safety can only be secured by taking care that at the various displacements and draughts of water a vessel may have in different conditions of loading, the centre of gravity is always kept at a proper depth below the corresponding points on the metacentric curve.

Such instability as may be due to deficiency, or absence, of metacentric height is not necessarily dangerous, and may not be sufficient to cause a complete capsize. It will, of course, cause the vessel to incline away from the upright, but a position of stable equilibrium may soon be reached; and the righting moments at greater inclinations, and the range of stability, beyond that point may be so large as to put all danger of upsetting out of the question if there are no openings through which water may find its way inboard, and no large weights free to shift. Many ships are in this condition when light, and some approach it when laden. On the other hand, there are vessels in which small metacentric height involves a serious risk of capsizing.

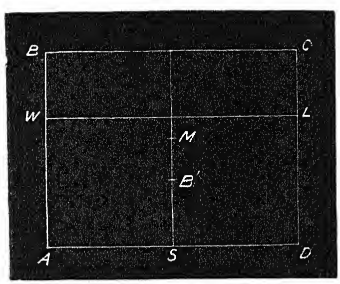
No fixed distance of centre of gravity below metacentre, or metacentric height, as it is commonly termed, can be adopted as a standard for application to all ships, because such a measure of stability is very imperfect and insufficient, and may by itself be misleading. This is chiefly due to the reasons that the form and proportions of the above-water part of the ship are not taken into consideration in the metacentric calculations, and the under-water form not completely so; and that the value, as a general measure of stability, of a given metacentric height is largely affected by the absolute heights in a ship of the centre of gravity and metacentre. The initial stability is, of course, constant for a given metacentric height whatever may be the absolute positions of the points M and G, but the righting moments at moderate and large angles of inclination, and the angle at which such righting moments vanish, or change into upsetting

moments, are largely dependent upon the absolute position of the latter point. Speaking generally, it may be said that, keeping the same distance between the metacentre and centre of gravity, the righting moments at successive angles of inclination, and the range of stability, are increased by lowering these points and reduced by raising them.

Experience proves that some classes of vessels are as safe and seaworthy in respect of stability with 1 foot, or even less, of metacentric height, as others are with 3 or 4 feet; while some of an exceptional character require much greater stability than even the latter figures would give. Examples of this class are to be found among ironclad monitors of very low freeboard and with heavy upper works, including armoured turrets and guns on deck; and also among paddle steamers of extremely light draught, with extensive tiers of houses above them. These are cases in which the metacentre and centre of gravity are both comparatively high in the ship.

The vertical position of the metacentre of a floating body is determined by the consideration that the height of the metacentre above the centre of gravity of the volume of displacement is equal to the moment of inertia of the plane of flotation about a longitudinal axis through its centre of gravity divided by the volume of displacement.

FIG. 2.



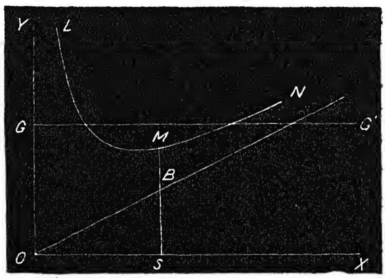
If ABCD (fig. 2) represent the transverse section of a rectangular prismatic body which floats in equilibrium with the side BC horizontal, WL the line of flotation, B′ the position of the centre of gravity of the displaced volume or centre of buoyancy, and M that of the metacentre, then by Bouguer's well known formula

$$B'M = \frac{\text{moment of inertia of plane of flotation WL}}{\text{volume WADL}}.$$

Let AB$=a$, BC$=b$, and WA, the depth of flotation$=d$. The moment of inertia of an unit of length of the water-line plane

$\mathrm{WL}=\frac{b^3}{12}$, and the volume of displacement of an unit of length of the prism $=bd$. Therefore $\mathrm{B'M}=\frac{b^2}{12d}$. But $\mathrm{B'S}=\frac{d}{2}$, and MS is therefore equal to $\frac{b^2}{12d}+\frac{d}{2}$. If a curve LMN (fig. 3) be constructed whose

FIG. 3.



abscissæ represent the various depths of flotation of the body, and ordinates the corresponding heights of the point M, this will be the curve of metacentres. OS is equal to the depth of flotation WA in fig. 2; BS is equal to the corresponding height of the centre of buoyancy B′ above S; and MS is equal to the height of the metacentre above S. It is obvious that the locus of the centre of buoyancy OB is a straight line whose equation is $y=\frac{x}{2}$. The equation to the curve of metacentres is $\frac{b^2}{12x}+\frac{x}{2}=y$, or $6x^2-12xy=-b^2$. This curve is therefore an hyperbola whose asymptotes are the axis OY,—which corresponds with the lower side AD of the section of the floating body in fig. 2,—and the straight line OB, which is the locus of the centres of buoyancy. The curves of metacentres for various geometrical forms of floating bodies possess many interesting properties, but it is foreign to the purpose of this paper to enter upon a full discussion of them. It may, however, be noted, as additional illustrations of these, that the ordinate of the metacentric curve at zero, *i.e.*, the one corresponding with no draught of water, is the radius of curvature of the transverse section of the floating body at its lowest point. Thus, for a body of circular section the height of the metacentre at the point where the draught vanishes is equal to the radius of the circle; for one the lowest point of whose section is angular, it is zero; and for one the bottom of whose section is straight and is parallel to the water-line, it is infinite.

When the body is completely immersed the metacentre is identical

with the centre of buoyancy, but if the upper surface is bounded by a plane which is parallel to, and coincident with, the water-surface, the curve of metacentres does not, when produced, end in the centre of buoyancy, as may be seen by fig. 3. The immersion of the plane of the upper surface BC causes a point of discontinuity in the curve of metacentres, which drops at once to the curve of centres of buoyancy. Curves of metacentres are given in fig. 7 for prismatic bodies of triangular and elliptical sections, and for a similar body the lower half of whose section is elliptical and the upper half rectangular (figs. 4, 5, and 6). The section in fig. 4 is an isosceles triangle with the base upwards and horizontal. In figs. 5 and 6 the major axis of the ellipse is horizontal. A comparison of the metacentric curves in fig. 7 will show how they are affected by changes in the form of the floating body. In the case of the triangular section the curve of metacentres is a straight line which passes through the immersed angle of the triangle.

Fig. 4.

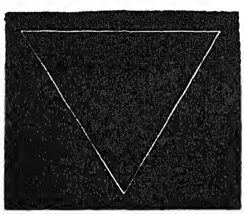


Fig. 5.

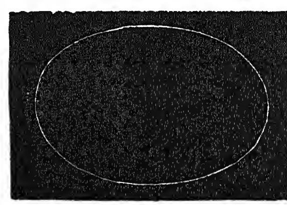
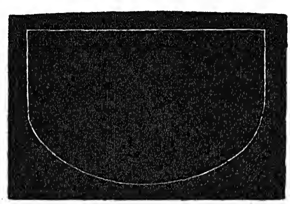


Fig. 6.

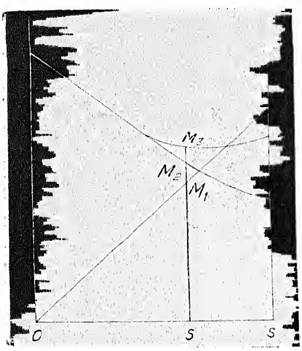


If OS represent any draught in fig. 7, then M_1, M_2, and M_3 are the positions of the metacentres at that draught for the three bodies under consideration; OS′ being the draught at which they become completely immersed.

If the rectangular floating body shown in fig. 2 be homogeneous, and the changes in its depth of flotation be caused by merely altering the density throughout, or by otherwise altering its weight so that the position of the centre of gravity remain the same, the latter will always be in the centre of the body. The locus of the centre of

gravity will therefore appear in fig. 3 as a straight line parallel to the axis OX. Let GG′ be this line. The equilibrium of the floating body will best able in the upright position for those depths of flotation at which the ordinates to the curve of metacentres LMN are greater than OG, and unstable when they are less. At the points where the curve LMN intersects GG′ the equilibrium will be neutral.

FIG. 7.



Triangular section.

Section whose upper half is rectangular and lower half elliptical, as in fig. 6.

Elliptical section.

In actual ships the locus of the centre of gravity is not a straight line such as GG′, any more than the curve of metacentres is a hyperbola like LMN; and the fundamental difference exists between them in practice, that whereas the curve of metacentres is constant for a ship, the locus of the centre of gravity is very variable in its character. The diagram in fig. 3 may serve, nevertheless, to illustrate the nature of the problem that has to be dealt with in investigating the stability of ships. The curve of metacentres for a ship can be readily constructed by applying Bouguer's theorem, viz., height of metacentre above centre of gravity of displaced volume $=\frac{\frac{2}{3}\int y^3 dx}{V}$, where y is the half-ordinate of the plane of flotation and V the volume of displacement. The integration of $y^3 dx$ is effected by one of Simpson's rules; and the volume of displacement and corresponding height of centre of gravity of displaced volume are computed by the same means. The curve of metacentres when once constructed is the same for all conditions of the ship, as it can only be altered by changes in her dimensions or form. In this important respect it differs entirely from the locus of the centre of gravity.

The locus of the centre of gravity of a ship is usually very irregular, and is neither fixed in character nor position. It varies with different weights and descriptions of loading; and, unlike the curve of metacentres, its ordinates cannot always be expressed in terms of the

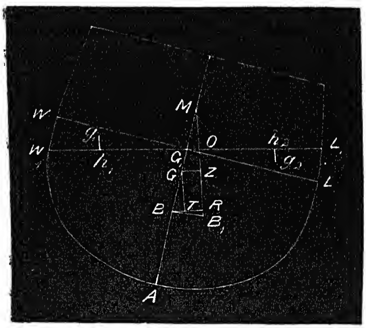
depth of immersion. What is usually done in practice, is to ascertain the position of the centre of gravity for such conditions of loading as may often be expected to occur; or to determine the limits between which it will lie in certain extreme circumstances. This variable and uncertain factor, of height of centre of gravity, is one of the greatest practical difficulties that have to be overcome in fully or accurately determining and regulating a ship's stability. The difficulty is not the same in all cases. In war-ships, for instance, and many which merely carry passengers and light baggage, the centre of gravity may either be regarded as fixed, or to vary with the draught of water in a specific manner which may be readily determined. On the other hand, many mercantile vessels employed in the carrying trade are laden with cargoes which, together with the coals that are required for consumption upon a voyage, weigh twice their own total weight. In such cases, the position of the centre of gravity of the laden ship is largely dependent upon stowage, and the stability may be entirely dependent upon it. The consumption of coal at sea introduces a cause of variation in position of centre of gravity, and metacentric height, which operates during a voyage; so that the stability may be materially altered after a steamer has left port, by reason of the consumption of coal. Such alteration frequently has the effect of diminishing the stability: and there are cases of steamers whose gross weight when fully laden is reduced at sea from this cause by over 25 per cent., and some in which the metacentric height is reduced by $1\frac{1}{2}$ feet. These large changes in the amount and distribution of a ship's weights—some of which take place at sea—sometimes make the problem of regulating the stability of a ship, so as to prevent its ever becoming deficient or excessive during her voyages, a very difficult and extremely delicate one.

The curve of metacentres and the positions of the centre of gravity for all possible draughts of water and conditions of loading are not sufficient, when obtained, for enabling the condition of a ship in respect of stability to be completely determined. Atwood showed, in two papers communicated to the Royal Society in 1796 and 1798, that much more than this is required. In his second paper, read on the 8th March, 1798, he says, "M. Bouguer, in his treatise entitled 'Traité du Navire,' has investigated a theorem for estimating the exact measure of the stability of floating bodies. This theorem, in one sense, is general, not being confined to bodies of any particular form; but in respect to the angles of inclination, it is restrained to the condition that the inclinations from the upright shall be evanescent, or, in a practical sense, very small angles. In consequence of this restriction, the rule in question cannot be generally applied to ascertain the stability of ships at sea, because the angles to which they are inclined, both by rolling and pitching, being of considerable magnitude, the

stability will depend, not only on the conditions which enter into M. Bouguer's solution, but also on the shape given to the sides of the vessel above and beneath the water-line or section, of which M. Bouguer's theorem takes no account." It may be added that Bouguer's theorem also neglects to take into account the volume of the above-water part of a ship, and to some extent the form of the below-water part; as well as the absolute height of the centre of gravity, which has been already referred to.

Atwood lays down a general theorem for determining the righting moments, at any required angles of inclination, possessed by a ship having a given draught of water and a fixed height of centre of

Fig. 8.



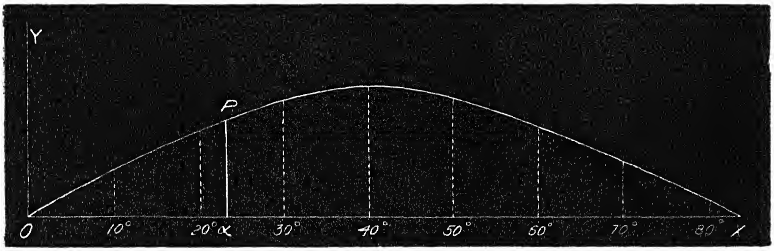
gravity. It is the following:—Let fig. 8 represent the transverse section of a ship which is inclined to an angle $WOW_1 = \theta$ from the upright water-line WL. Let G be the position of the centre of gravity; B the centre of gravity of the volume of the displaced fluid—or centre of buoyancy—when upright; and B_1 the position to which it has been moved by the inclination of the vessel through the angle θ. The original form of the under-water volume WAL has now been changed by the addition of the wedge-shaped piece LOL_1 and the deduction of the wedge-shaped piece WOW_1. The volumes of these wedges must be equal, because the displacement has not been changed by the mere act of inclination. Let g_1 be the centre of gravity of the wedge WOW_1, g_2 that of LOL_1, and v the volume of each wedge. Then the horizontal shift, BR, of the centre of buoyancy $\times$ the volume of displacement, or $V \times BR, = v \times h_1 h_2$. But $BR = GZ + BG \sin\theta$, and therefore $GZ = \frac{v}{V} \times h_1 h_2 - BG \sin\theta$.

This is the formula by which the stability of a ship at various angles of inclination is ordinarily computed; GZ being the arm of the couple at the ends of which the weight of the ship and the

upward pressure of the water act, either to restore the vessel to the upright position or to produce further inclination. The factors v, V, and $h_1 h_2$ are readily calculated from the external dimensions and form of the ship by means of Simpson's rules; the position of the point B, the centre of buoyancy in the upright position, is similarly obtained; and G is either determined by experiment, or by calculating in detail the weights and statical moments of the component parts of the structure and loading. Mr. F. K. Barnes, one of the present Chief Constructors of the Navy, read a paper before the Institution of Naval Architects, in 1860, in which he showed how the requisite calculations could be made concisely and with facility.

Notwithstanding Atwood's demonstration of the imperfect and unreliable standard of stability furnished by mere metacentric height, and his theorem for enabling the righting moments at large angles of inclination to be determined, the step which it may now appear would naturally follow was not actually taken till 1867. In that year a question arose at the Admiralty respecting the stability of some low freeboard monitors at very large angles of inclination; and Sir E. J. Reed, then Chief Constructor of the Navy, directed the matter to be investigated. It was placed in the hands of Mr. William John, who made the calculations, and embodied them in the graphic form now known as the curve of stability.

Fig. 9.



Thus in fig. 9, if OX be an abscissa line, upon which the various equal divisions represent angles of inclination of a ship, and any ordinate, such as Pα, be the length of GZ (see fig. 8) at the angle of inclination α, OPX will be the curve of stability for the particular draught of water and position of centre of gravity under consideration. The results of Mr. John's calculations were described in a paper read by Sir E. J. Reed before the Institution of Naval Architects in 1868; and a further paper containing an improved method of applying Atwood's theorem to the calculation of stability upon this extended scale, was read before the same Institution by Messrs. John and White, in 1871.

The curve of stability, as thus constructed, has been in common use at the Admiralty, and in a few of our leading mercantile ship-yards for some years. The loss of H.M.S. "Captain," by capsizing at sea, furnished the impetus which led to the practice of producing stability information in this complete and instructive form, becoming established at the Admiralty. Many losses have occurred of late years in the mercantile marine from a similar cause, and forcibly directed the attention of mercantile naval architects to the same point. Curves of stability have been constructed for large numbers of vessels of various classes, many of which have been published; and the general character of a ship's stability can now be judged of with much greater accuracy than was possible a few years ago. It appears that prior to 1867 no calculations had been made which showed how the stability of a ship became affected by inclining her till the water-line came up over the deck; or at what angle the stability vanished. Messrs. John and White say in the paper before referred to: "The metacentric stability, as it was termed, was by general consent taken as a sufficiently good standard of comparison, and no approximation was made, nor any great importance attached to the angle of inclination when a ship ceased to be stable. It was very generally known that up to very considerable angles of heel, the stability of high-sided ships continued to grow, even more rapidly than it appeared to do from the metacentric method, and the vague impression that the angle would be very large at which the ship became unstable, was considered sufficient to render investigation needless."

The investigations conducted at the Admiralty into the stability of war-ships of low freeboard, and those made by naval architects outside of the Admiralty into the condition of deeply laden merchant steamers of low freeboard and with high centres of gravity of cargo, prove that the mere application of the metacentric method may often lead to a false sense of security being established respecting the stability of certain types of vessels.

H.M.S. "Captain" had a metacentric height of about 2½ feet when laden, which—in the absence of definite information, at that time most unusual and not considered absolutely necessary, respecting the righting moments at large inclinations, and the angle at which the stability vanished—was not supposed to be insufficient. Some of our low freeboard monitors and deeply laden merchant vessels with flush decks and low freeboards, have metacentric heights which, by themselves, furnish no clue to the rapidity with which the stability diminishes after a moderate angle of inclination has been passed; or the smallness of the angle at which it vanishes. The introduction of curves of stability, and the extent to which they have been applied in practice, have led to a due and precise appreciation being formed, by many, of the dangers attendant upon low freeboard. The

opinions previously existing upon the point, which were usually based upon mere surmise or vague impression, and often influenced by prejudice, can now be corrected by means of exact and conclusive data. The effect of low freeboard upon stability has latterly been largely made known and discussed.

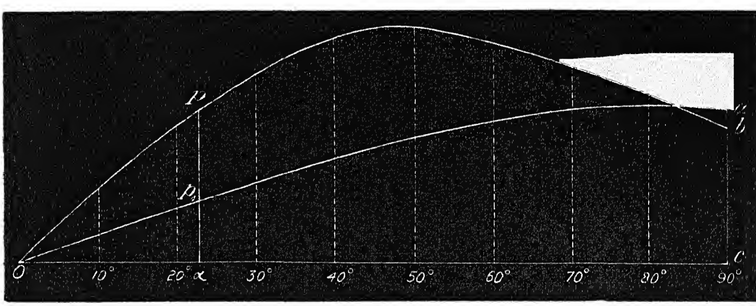
It has been seen that the mere act of setting off the righting moments obtained by Atwood's formula, at various angles of inclination, as ordinates of a curve, and thus obtaining a complete graphic representation of the variation of righting moment with angle of inclination, threw a flood of light upon the general problem of a ship's stability, and enabled it to be far more comprehensively and accurately treated than before. It also enabled definite and instructive generalisations to be made which were previously impossible. It will be observed, however, that the curves of stability referred to only deal with the matter so far as certain fixed draughts of water and positions of centre of gravity are concerned. A curve of stability for a given draught of water and position of centre of gravity ceases to be applicable if changes are made in the weight and consequent draught of water of a ship, or the position of the centre of gravity, or both.

In the case of war-ships, curves of stability are usually constructed for three conditions. 1st, for the draught of water and position of centre of gravity of the vessel when fully laden; 2nd, the same, but with all coal consumed; and, 3rd, when quite light, and without any coal, ammunition, or consumable stores. In certain cases there may be special conditions for which additional curves are required, but usually the above are all that are considered necessary. The stability at intermediate points is not often supposed, or found in practice, to vary sufficiently to call for further attention. In mercantile steamers, however, which are often launched in a very light condition and are constructed for carrying heavy cargoes, and large supplies of coal, the question of stability requires a more exhaustive, and somewhat different, mode of treatment. It becomes necessary, in fact, to make another step in the same direction as was taken when the righting moments for various angles of inclination, described by Atwood, were represented as a whole for a fixed draught of water and loading by means of a curve of stability. What is still further required to complete the representation of a ship's stability is to show how the curves of stability themselves vary with draught of water and position of centre of gravity, and to record this variation in a form that will render it easy to construct curves of stability for any specific draughts of water and positions of centre of gravity.

The curves of stability of merchant steamers are required for so many different draughts of water and positions of centre of gravity, that I have found it convenient in practice, after obtaining curves

for the extreme light and loaded conditions, and one or two intermediate ones, to construct cross-curves, from which the ordinates of a curve of stability at any draught between the two extreme ones can be readily measured off. In order to do this, the curve of stability already described has to be analysed, and dealt with under a different form. The ordinary curve of stability, as illustrated in fig. 9, is a curve showing the variation in the length of GZ in fig. 8—or the arm of the inclining couple—with the angle of inclination. Pα in fig. 9 is the length of GZ in fig. 8, at the angle of inclination α. But $GZ = BR - BT = BR - BG \sin\theta$. Therefore P$\alpha$ in fig. 9 is the difference of these two quantities at the angle of inclination α. The curve of stability, or curve of GZ, is a curve whose ordinates are equal to the differences between the corresponding ordinates of two curves, one of which represents the variation in the length of BR with change of inclination, and the other is a curve of sines with

FIG. 10.



radius BG. Thus if Opb, fig. 10, be a curve, of which the ordinates, such as $p\alpha$, are equal to the length of BR at the angle of inclination α; and Op_1a a curve of sines from 0° to 90° with radius $ac = BG$; then the differences between the ordinates of these curves will give the corresponding values of GZ, or the ordinates of the curve of stability. Thus pp_1 is the length of GZ at the angle of inclination α, and is equal to the ordinate Pα of the curve given in fig. 9.

It will be obvious that after calculating three or four curves of BR for a vessel, including those for the two extremes of draught, no further calculation is necessary for obtaining a similar curve at any intermediate draught, since it is only requisite to construct cross-curves at given angles of inclination—say at intervals of 10°—and from these cross-curves to measure off the ordinates of the curve BR for the draught required. Having thus obtained the curve of BR for the draught in question, the ordinary curve of stability,

or lengths of GZ, can be obtained by setting off a curve of sines as in fig. 10 to radius BG, and deducting the ordinates of this curve from the corresponding ones of the curve of BR. This mode of constructing curves of stability has been rendered necessary by the great variations in draught of water and position of centre of gravity that have to be dealt with in many mercantile steamers. The demand that has thus arisen for constructing cross-curves of stability, *i.e.*, curves showing the variation of righting moment with draught of water at constant angles of inclination, has recently led to greatly improved and more rapid methods of calculation being devised. Amsler's mechanical integrator has been invaluable in bringing about this desirable result, and it is now possible to make a complete set of cross-curves of stability for a ship in a few days.*

In considering the stability of a ship from the point of view of variation of righting moment with draught of water, the angle of inclination being constant, instead of from that of variation of righting moment with angle of inclination, the draught being constant, as was formerly done; or, rather, in considering the subject from both points of view instead of almost exclusively from the latter, several interesting and important results are obtained, which do not appear to have received the attention they deserve.

It has already been stated that one of the first lessons taught by the introduction of curves of stability was the decrease in the righting moments at large angles of inclination, and the rapidity with which stability frequently vanishes altogether, in vessels of low freeboard. These are matters of great practical importance, and have attracted a great deal of attention, on account of the numerous vessels of various types which are more or less affected by them. The connexion between low freeboard and range of stability, and the manner in which the latter is affected by metacentric height and the position of the centre of gravity, has latterly been well discussed and explained;

* The method thus described of calculating a series of ordinary curves of stability and afterwards constructing cross-curves from them is tedious and complicated. It is simpler and very much more expeditious to calculate the cross-curves directly by applying the integrator to the under-water part of the ship, instead of to the wedges of immersion and emersion; and thus determining at once the positions of the vertical lines through the centres of buoyancy at the required angles of inclination. By this means the necessity is avoided of calculating separately the volumes of the wedges of immersion and emersion, and of correcting the positions first assumed for the inclined water-lines in order to make these wedges approximately equal. Mr. William Denny, of Dumbarton, was the first to call my attention to this very important and useful simplification; and it was described by him in a paper read before the Institution of Naval Architects in April last. Other investigators have also been working in the same direction, and several papers dealing with the extension and simplification of stability calculations will be found in the "Transactions of the Institution of Naval Architects" for 1884, including one of great interest and value by M. Daymard.—25th July, 1884.

and the dangers which may be incurred by low-sided ships are well understood by many. In former times, it was not so necessary as it has recently become, to carry theoretical investigations to the point of ascertaining the angle at which a ship would capsize. It is only during the last twenty years that small height of side out of water has been thought a desirable quality for sea-going war-ships to possess, or that certain classes of merchant vessels have been evolved in which range of stability has been unduly limited by lowness of freeboard. When Atwood wrote his papers, and for very many years after, war-ships were built with such lofty sides, and merchant vessels were so comparatively uniform in type, and so deep in proportion to their other dimensions, that no demand arose for complete curves of stability. Besides, all the vessels of Atwood's day, and for long after, were sailing ships; and such few investigations as were made respecting their stability, were for the purpose of determining their sail-carrying power. The constructors of the old types of vessels judged of and regulated stability by reference to the practical test of sail-carrying power at sea, and this was usually sufficient for the purpose. Modern variations of type, which began after the introduction of steam, have for some time, however, rendered scientific calculations necessary, which before were considered merely interesting or curious. The many departures from the comparative uniformity of proportions and form that once prevailed, and particularly the extent to which some of these departures have gone in the direction of reducing freeboard, have created the necessity referred to.

Curves of stability, having been first produced for the purpose of ascertaining the effect of low freeboard, and having disclosed the dangers which lowness of freeboard may cause, have been largely used for that purpose. If we consider the cross-curves of stability—or curves of righting moments for various draughts of water, the angle of inclination being constant—it will be seen that while the upper and middle portions of those curves have been often dealt with, the lower portions have been almost, if not quite, neglected. The stability of a floating body at light draughts is, however, similar in character to that at deep draughts, and the same peculiar features and dangers that have been found to exist with low freeboard are frequently connected also with lightness of draught.

It fell to my lot to make some investigations respecting the stability possessed by the "Daphne" at the time of the disaster which befell her, and to give evidence respecting the same. I afterwards, by way of explanation of a portion of the evidence, wrote a letter to the "Times," which appeared on the 1st September last, calling attention to the relation which exists between the righting moments at deep and light draughts in certain elementary forms of floating bodies. The proposition I then enunciated, which illustrates

the point under consideration, is the following: If any homogeneous body, which is symmetrical about the three principal axes at its centre of gravity, be of such density as to float with its lowest point at a depth x below the water; then, if the density be altered so as to make it float with its highest point at a height x above the water, the righting moments will be the same in both cases at equal angles of inclination, and consequently the range of stability and complete curve of righting moments will be the same. This proposition can be made still more general, as was shown by Mr. William John, in a letter to the "Times" of the 5th September; as it applies to all homogeneous floating bodies of irregular form revolving about a horizontal axis fixed only in direction. In this general form, the condition of turning the body through an angle of 180°, or upside down, must, however, be included, because the immersed volume in the one case must be of the same form as the emersed volume in the other, and this can only be obtained with irregularly-shaped figures by turning them through an angle of 180°. For this reason I chose symmetrical figures for the purpose of giving a popular illustration of the analogous effects of low freeboard and light draught upon the stability of ships, and avoided introducing the condition of turning through 180° in order to get similar volumes above and below water in each case. The general proof of the proposition laid down is that the line joining the centre of gravity of the immersed volume with that of the volume above water must pass through the centre of gravity of the whole body, and the distances of the centres of gravity of the two sections from that of the whole body are inversely proportional to their volumes; so that the moment of stability, which is proportional to the immersed volume multiplied by the distance of its centre of gravity from that of the whole body, will be the same in both cases.

As the righting moments at equal angles of inclination at the deep and light draughts described are the same in a homogeneous floating body of symmetrical form, it follows that at the same draughts the lengths of GZ, or the arms of the righting lever at equal angles of inclination, and also the metacentric heights, are in the inverse ratio of the displaced volumes.

The moment of stability $V \times GZ$ is by Atwood's formula, see fig. 8, equal to $v \times h_1 h_2 - V \times BG \sin \theta$. Now $v \times h_1 h_2$, which is the moment of the wedges of immersion and emersion, is the same whether WAL be the below-water or above-water volume; the immersed wedge in the one case being the emersed in the other, and *vice versâ*. $V \times BR$ is therefore the same in floating bodies of any form that revolve about a horizontal axis fixed in direction, whether WAL be the above-water or below-water volume. For such bodies as are homogeneous $V \times BG \sin \theta$ is also equal in the two cases, because

BG is inversely proportional to V, or the immersed volume; from whence we again derive the result that the moments of stability at equal angles of inclination are the same. In the case of a body which is not homogeneous, and in which the centre of gravity is at a distance GG_1 from G, the centre of gravity of a similar homogeneous body, the moments of stability at equal angles of inclination when WAL represents above-water and below-water volumes respectively, differ from each other by an amount equal to $(V_1+V_2)GG_1 \sin\theta$, where V_1 and V_2 are the two volumes into which the whole body is divided by the water-line plane WL. When the change of immersed volume is produced by merely increasing the depth of immersion, as in a ship—and not by rotating the body so as to make WAL represent above-water and below-water volumes alternately—the difference between the moments of stability is $(V_1-V_2)\ GG_1 \sin\theta$. In the first case it is $GG_1 \sin\theta \times$ the whole volume of the body: and in the second it is $GG_1 \sin\theta \times$ the difference between the volumes into which the body is divided by the water-line plane WL.

Some of the results which follow from the above considerations have been previously noticed. Atwood, in his paper read before the Royal Society in 1796, discusses at great length the positions of equilibrium of homogeneous rectangular bodies, and prisms of square sections, with varying specific gravities. He shows that whether the specific gravity of a square parallelopiped be a or $1-a$, it will float in equilibrium with its faces at the same angles to the water-surface, and will pass through the same number of positions of equilibrium in turning through 360°. These cases are included in the general proposition that the righting moments at equal angles of inclination are the same for both densities of the body, because the righting moments will vanish, *i.e.*, positions of equilibrium will occur, at equal angles of inclination. Atwood also shows, in a paper read before the Royal Society in 1798, that the stability of a vessel whose sides are inclined at a given angle below the water-surface, is equal to that of a vessel whose sides are inclined to similar angles above the water-surface; the breadth of the water-line and other conditions being the same in both cases. He goes on to say that this proposition is not confined to the case which he demonstrates, but is equally true whatever figure be given to the sides of the ship, and whether they are plain or curved, provided that the sides under the water in one vessel are similar and equal, and similarly disposed in respect of the water-line, to the sides of the other vessel above the water-line. The unnecessary condition, so far as homogeneous bodies are concerned, introduced by Atwood, that the displacements shall be equal, prevented him from pushing his conclusions to the extent to which they have been carried in this paper.

It should here be remarked that in dealing with cross-curves of

stability, and thus considering the variation of stability with draught of water, the curves of righting moments require to be constructed, and not merely curves of GZ, or lengths of righting arm. The ordinary curve of stability usually has for its ordinates the lengths of GZ at the various angles of inclination. This is right enough for the condition under which such curves are constructed; because the displacement is then constant, and the curves represent either lengths of righting arm or righting moments, according to the scale upon which the ordinates are measured. In the cross-curves of stability, however, draught is one of the variable elements, and the displacement changes with it. A cross-curve whose ordinates represent the lengths of righting arm at various draughts of water is therefore quite different in character from a cross-curve of righting moments, whose ordinates are lengths of righting arm × displacement. It is necessary, in order to judge accurately of the variation of stability with draught of water, to use curves of righting moments, and not merely curves of GZ, such as are usually considered sufficient when the draught of water is fixed.

FIG. 11.

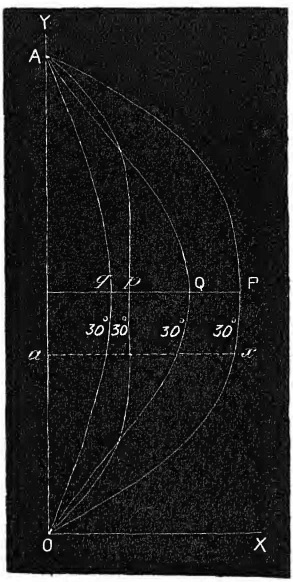


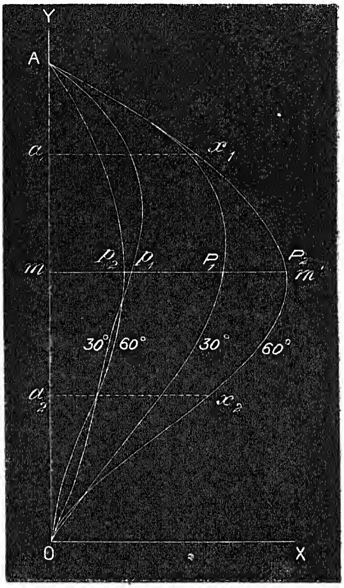
Fig. 11 shows cross-curves of stability, at angles of inclination of 30°, for two homogeneous floating bodies of prismatic form, and of the same breadth and depth; one being rectangular and the other ellip-

tical in cross section. The shorter axis is vertical when the bodies are upright, and is two-thirds of the longer axis, or extreme breadth. The measurements in the direction of OY give the depths of immersion, and those in the direction of OX represent moments. The curve APO gives the values, for the rectangular body when inclined to an angle of 30° from the upright, of the horizontal shift of the centre of buoyancy multiplied by the immersed volume, or $\text{BR} \times \text{V}$, see fig. 8. Thus if O*a* be any draught of water, the ordinate *ax* gives the value of $\text{BR} \times \text{V}$ at that draught. The curve AQO is the corresponding curve of moments for a prismatic body of elliptical section, and of equal length to the above, when inclined also to an angle of 30°. The axes of the ellipse are of the same length as the sides of the rectangular section taken in the former case; the minor axis being two-thirds of the major, and the minor axis being vertical when the body is upright. A*po* and A*qo* are the corresponding curves of $\text{GZ} \times \text{V}$ at the various draughts of water, and are obtained by deducting $\text{V} \times \text{BG} \sin 30°$ from the ordinates of the curves APO and AQO. The bodies being homogeneous, G is taken at the middle of the depth. These curves therefore represent the actual righting moments of the two bodies under consideration, for an inclination of 30°, at all draughts of water. The ordinates measured to the right of AO giving righting moments, and those to the left, if there were any, would be upsetting moments. It will be seen that the whole of the curves in fig. 11 are symmetrical with respect to a line drawn parallel to OX at one half the depth of total immersion. The elliptical prism tends to return to the upright, when at the inclination of 30°, at all draughts of water; and exerts the maximum effort to do so when immersed to the middle of its depth. The rectangular prism, when inclined to the same angle, also tends to return to the upright at all depths of immersion; the maximum righting moment is not, however, obtained when floating at the middle of its depth, but at draughts which are at equal distances above and below it.

Fig. 12 represents similar curves for a prismatic body, the upper half of whose section is rectangular, and the lower half elliptical as shown in fig. 6; the extreme dimensions of the section being the same as in the previous cases. This form of section is an example of the kind of departure from symmetry of form which exists in ships. It has been seen that if homogeneous bodies of symmetrical form be altered in density so as to float alternately at water-lines which are at equal distances above and below the centres of such bodies, the righting moments at equal angles of inclination will be the same at each draught. In the body for which the curves in fig. 12 have been constructed, the departure from similarity of the immersed and out of water volumes causes a difference between the righting moments at the draughts described. AP_1O and AP_2O represent the curves of

$BR \times V$ at angles of inclination of 30° and 60° respectively; and Ap_1o, Ap_2o are the corresponding curves of $GZ \times V$, or curves of righting moments, when G is taken in the position it would occupy if the body were homogeneous. The lines a_1x_1 and a_2x_2 indicate draughts at

FIG. 12.



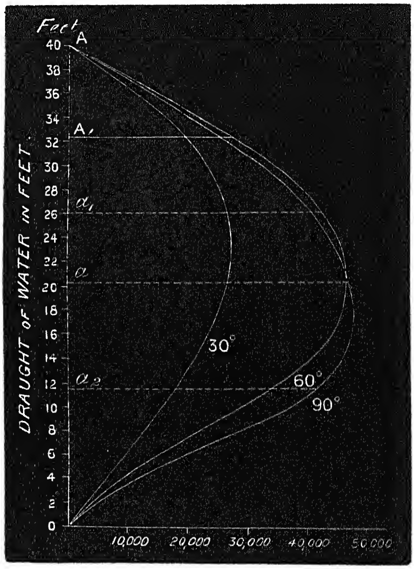
which equal volumes are cut off above and below water, and mm' shows the depth at which the immersed portion is one-half of the total volume of the body.

It will be seen that the righting moments are greater at 30° and 60° of inclination when the body is deeply immersed than when it is floating at light draughts with equal volumes below water to those which are above in the other case. The relation between the righting moments at the two extremes of draught is, however, largely determined in a ship by the position of the centre of gravity, which in this case has been taken as for a homogeneous body. This will be seen by the next example.

Fig. 13 gives curves of $BR \times V$ for an actual ship, at 30°, 60°, and 90° of inclination respectively. The vessel for which these have been constructed is 400 feet in length, 44 feet in breadth, and 32 feet 6 inches in moulded depth. The extreme depth from the top of keel to the highest point of the sheer of the upper deck is 40 feet. The point O is the top of the keel, A is the highest point of the sheer of the deck, and A_1 the lowest point of the upper deck at side, from

which freeboard is measured. The horizontal ordinates of these curves represent the moments BR × V, at the draughts to which they correspond; the scale of moments in foot-tons being shown upon the base line. The displacement of the vessel when wholly immersed

Fig. 13.

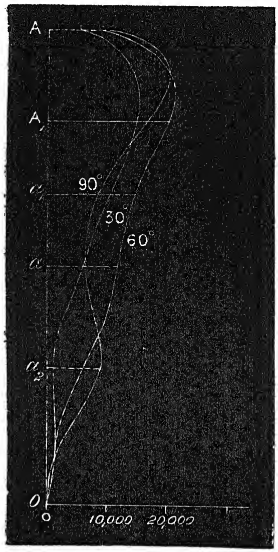


is 11,800 tons, and when displacing half this amount—or 5,900 tons—she draws 20 feet 6 inches of water; and this depth of flotation, with the corresponding values of BR × V, are shown by the ordinate drawn at the point a. a_2 represents the draught of water at which the vessel was launched, and a_1 the draught at which there is an equal volume out of water to that below water at the draught a_2. The draught of water at the point a_2 is 11 feet, and the freeboard above the point a_1 is 7 feet.

If the centre of gravity be taken at 19 feet above the top of keel for all draughts of water—it always varies, and in some cases considerably, with the draught, as has been stated, but 19 feet is found to be a fair mean height for the ship in question—and the moment V × BG sin θ be deducted from the ordinates of the curves in fig. 13, we obtain new ordinates, which represent the curves of righting moments, V × GZ; and these are shown in fig. 14. It will be seen that the righting moments which correspond with the ordinates of the usual curves of stability are much larger at deep draughts than at light draughts. For instance, the ordinates of the usual

curve of stability for the launching draught of 11 feet at a_2 give very much smaller moments than the curve for the deep draught at a_1, where there is an equal volume above water to what there is below in the other case, and the freeboard is only 7 feet.

FIG. 14.

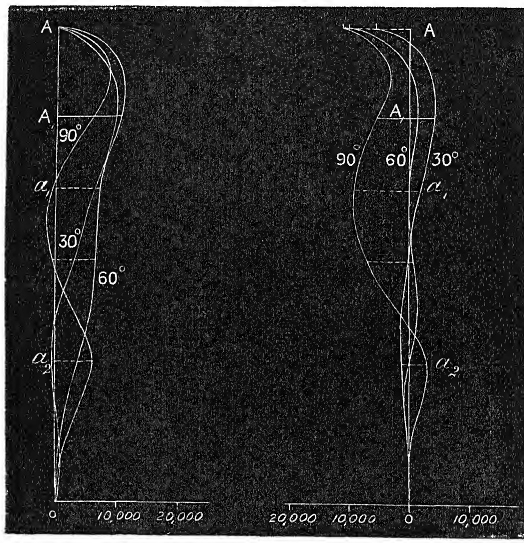


The centre of gravity taken in fig. 14 is 1·1 feet below where it would be if the external surface of the ship inclosed a homogeneous volume. Its position with reference to that of the centre of gravity of a homogeneous body of the same form largely determines the stability at all except small angles of inclination, and also the relation between the stability at light and deep draughts. Figs. 15 and 16 show what fig. 14 becomes changed into, if the centre of gravity is first raised 1·1 feet, so as to be in the same position as if the ship were a homogeneous body, and, second, if it is raised a further 1·1 feet, so as to be as much above this point in fig. 16 as it is below it in fig. 14. A comparison of these figures will show that, except in the case of a very high centre of gravity, the stability at light draughts with various positions of centre of gravity is less than at deep draughts.

It appears, therefore, that in the case of this ship, and she is a type of many mercantile passenger steamers, the proposition respecting the equality of the stability at light and deep draughts which has been shown to apply to homogeneous symmetrical bodies, requires

modification in a direction which is disadvantageous to light draughts. When there are equal volumes above and below water in this vessel, the righting moments at the light draughts are generally much less than at the deep draughts, except when the centre of gravity is raised excessively, and, for this class of ship, unusually high.

FIG. 15. Fig. 16.



The analogy that exists between light draught and deep draught stability in floating bodies of approximately symmetrical forms, and particularly the point of resemblance afforded by the fact that what is a wedge of immersion in one case is that of emersion in the other, and *vice versâ*, cannot fail to have struck some who have had to calculate the stability of bodies floating at light draughts, but attention has never been prominently called to it. It is desirable, however, that the connexion between the two cases should be fully realised, and the dangers peculiar to very light draught of water appreciated as thoroughly as are those which attach to low freeboard. Just as was said in 1871, in a passage already quoted, that metacentric height was formerly, by general consent, taken as a sufficiently good standard of comparison in judging of the stability a ship would have, on account of "the vague impression that the angle would be very large at which the ship became unstable," so, since the introduction of curves of stability, the dangers attaching to light draught have been frequently lost sight of, because of the equally vague impression that, so long as a vessel has a high side out of water, and any

metacentric height, she will have large righting moments at great inclinations and a large range of stability. The investigations made at the time of the "Daphne" disaster, and the discussions which these and the elaborate report of the Government Commissioner, Sir E. J. Reed, have caused, now place this question upon a somewhat different footing; but at the time referred to the general belief appears to have been as stated.

The considerations set forth in this paper are mainly intended to draw attention to the necessity for taking a more comprehensive view of the problem of stability than has formerly prevailed, by investigating the cross-curves of stability of ships, and thus ascertaining how the righting moments at fixed angles of inclination vary with draught of water. They also aim at showing how the stability at fixed inclinations does vary in some ships with draught of water, and becomes comparatively small at light draughts. In designing ships and other structures which are required to float safely at very light draughts of water, such calculations are necessary if accidents are to be prevented. In some cases the necessity is as great, or even greater, than for vessels of low freeboard.

The subject is so extensive that I feel quite unable to attempt any exhaustive treatment of it at the present time. All that has now been possible is to call attention to some of its main features and to show why it requires to be followed up and thoroughly investigated.

"Notes on the Varieties and Morphology of the Human Lachrymal Bone and its Accessory Ossicles." By A. MACALISTER, F.R.S., Professor of Anatomy in the University of Cambridge. Received March 14, 1884. Read March 27.

[PLATES 1—3.]

Having examined 1000 lachrymal bones and the soft parts of the lachrymal region of over 300 orbits, I have compiled therefrom the following notes on the anatomy of these parts. The lachrymal is one of the most variable bones in the human skull, and one of the most perishable, being frequently destroyed by careless or ignorant methods of handling crania.

I. I have notes of two* instances of deficiency of the lachrymal bone. In one of these cases the whole wall of the groove is formed by the nasal process of the maxilla in front, and by the ethmoid and

* I have since seen eight additional instances of absence, in the Hunterian and other collections, five being in Hindoo crania, two in Negroes, in none of which was there any trace of synostosis.

frontal behind; a small lacuna closed by membrane completing the boundary. In the second instance the right groove is entirely formed by the maxilla, the nasal process of which is expanded backwards (fig. 20, *x*) into a flat plate behind the crista lachrymalis posterior (*y*), which also is maxillary. This plate articulates above with the frontal, and posteriorly with the prolonged orbital plate of the maxilla, which meets the frontal in a suture between the back of the nasal lamella and the front of the os planum. On the left side the lobe of the nasal process articulates by suture with the ethmoid, and below it there is a small lacuna (fig. 28, *lac.*), which may possibly have been filled by an ossicle. In this skull all the sutures are distinct, so the variety is not due to synostosis.

Systematic writers speak indefinitely of such cases, and Krause ("Handbuch," III, p. 71) says that it occurs in 8 per cent. of crania, but does not specify from what number of crania his percentage is derived. This is not my experience, and I believe it is not safe to make museum crania* the basis of such observations, as I have more than once seen instances of rudimental lachrymal bones in which the diminutive bone fell out in the course of handling. Metzger also records cases of absence ("Curat. Chirurg. Fistulæ Lachrymalis, Hist. Critica," Münster, 1772, p. 66).

II. Rudimental states of the bone are very common, and I think it probable that many of the so-called instances of deficiency are really examples of some one or other of these conditions. I have notes of five forms of rudimental bone.

α. Development of an orbital plate alone, terminating at the crista lachrymalis, and with consequent exclusion from the lachrymal groove which is formed of the nasal process of the maxilla alone. This I have thrice seen, all in unsymmetrical examples; the bones in each case were simple scales of small size, corresponding to the hinder portion of the orbital plate of the normal bone, in one case resembling the superior ethmo-lachrymal ossicle to be after described.

In several crania in which the lachrymal bone had been broken or had fallen away, I have seen the largely developed lachrymal lamella of the nasal process of the maxilla which, in the perfect cases, was associated with this variety, and hence probably they were instances of the same condition. A similar arrangement is described by Henle ("Knochenlehre," p. 193) as occurring in an Italian skull.

β. Development of two detached ossicles in place of a normal bone. This I have once found in a Negro skull. The lower of the two ossicles was the larger, and formed a small part of the lachrymal

* Another reason why museum crania form an unsafe basis for statistics is the obvious one that they are to a large extent selected on account of their exhibiting peculiarities.

groove, as well as the lower part of the orbital plate behind it, the upper corresponded to the top of the orbital plate of normal lachrymal bone; they are separated by a membranous interval. Possibly this should be classed with the cases, to be after mentioned, of superior ethmo-lachrymal ossicles, but the upper bone took the place of the upper fourth of the crista lachrymalis, and so differed from all the instances of ossicle with which I have met.

In one other instance of divided lachrymal the two parts were separated, not by a membranous space, but by a projecting ridge of the os planum which traversed the lachrymal and touched the frontal. This was an instance of the overgrowth of the wall of the partition between the upper and lower lachrymo-ethmoidal cells jutting through the surface scale of lachrymal. Professor Gruber has described a somewhat similar case ("Müller's Archiv," 1848, p. 412, T. xiv). In a South American skull I have seen a similar spur traversing the lachrymal as far as the crista.

γ. Development of a narrow string-like lachrymal along the crista, with a very narrow lachrymal part in front, and a still narrower orbital slip behind; this, which is the persistence of an early embryonic form, is rare. I have notes of three cases only. In one of these the bone was not more than 2·5 millims. broad, and lay along the front of the ethmoid. In the other the strip was closely perforated with holes, another embryonic character; in neither was there any *hamulus lachrymalis*.

δ. In rather more than 2 per cent. the bone is composed of a coarse reticulum whose meshes in the recent state are closed by membrane; most of such cases are old bones, and the holes are due to absorption consequent on dilatation of the ethmo-lachrymal cells, but in some of those examined the same condition occurred in young crania, and in most of the recent orbits in which such deficiencies were noticed the bone had a continuous sub-periosteal membrane over these holes. Ten of these bones were reduced to a few trabeculæ which were the remains of those parts of the normal bone which are thickest and densest, viz., the crista and the rods between the upper and lower lachrymo-ethmoidal cells, and a bar along both the superior and the inferior margins.

In five of these cases of deficient lachrymal bone islands of the ethmoid filled in the holes; two of these were very old crania.

ε. In several crania, three at least, a space closed by membrane existed between the back of the lachrymal and the anterior margin of the os planum. In one I found a similar membrane-closed space between the front of the lachrymal and the back of the nasal process of the maxilla; both of these were much fenestrated bones.

In all these cases of fenestration the deficiencies of the bone were opposite the middle of the lachrymo-ethmoidal cells, and the reticular

bars which represented the bone were thus the rods representing the cell boundaries.

III. Variations in outline. No two lachrymal bones are exactly alike, and all intermediate forms between the broadly ovate and the narrow rectangular have been found. The patterns of the borders also are the subjects of great variety, from perfect straightness to the most irregular lobulation. The anterior edge is always the longest, the superior is the shortest, the inferior is usually the least toothed and the most uniform in outline.

The upper or frontal edge presents three types; it is nearly horizontal in 31 per cent., it rises obliquely from behind forwards to the maxilla in 21 per cent., but in the majority, 43 per cent., it formed an angle salient upwards and forwards, as the upper edge of the orbital portion rises obliquely to the top of the crista, and thence the upper edge of the grooved portion descends rather steeply to the maxilla. In only one bone did the entire border slope downwards and forwards.

This border is approximately on the level of the top of the os planum in 48 per cent., rises above it in 39 per cent., but lay below it in 13 per cent., the edge is usually denticulated, especially at the top of the crest, and from this forward where it is thicker than it is behind. Ankylosis of this border is not uncommon, to some extent it occurred in one out of every thirty examined, especially in front of the crista.

A spur of the frontal descended between the lachrymal and the os planum in 34 per cent., but this was only large in one-third of these. A similar frontal spur projects into the maxillo-lachrymal suture in two-ninths of those examined, but was usually slight, only strongly marked in one-fourteenth of these cases of its occurrence. The best marked example of the post-lachrymal spur projected downwards for 5 millims., and of the pre-lachrymal for 4·5 millims.

The upper edge is below the level of the maxillo-frontal suture in all, and this suture usually rises obliquely in front of it, but in some (percentage not recorded) it rises suddenly and steeply.

The ethmo-lachrymal suture is concave backwards in 50 per cent. straight in 12 per cent., irregular in 36 per cent., convex backwards in 2 per cent., it is more or less toothed in 25 per cent., and seldom ankyloses to the ethmoid ($1\frac{1}{2}$ per cent.) The posterior superior angle is usually rounded and seldom projects as far backward as the posterior inferior; in one it sent a process backwards above the ethmoid for 2 millims., a similar condition of the posterior inferior angle undershooting the ethmoid occurred to a marked extent in 2 per cent., and to a slight extent in 14 per cent. A slight maxillary spur projects between the ethmoid and os planum in 14 per cent., but is only sharp and strong in 3 per cent. The anterior edge is usually irregular and convex forwards, seldom straight $1\frac{1}{2}$ per cent.) or

concave (1 per cent.) This is the margin which is the most frequently ankylosed, and was more or less synostosed to the maxilla in 4 per cent. In these cases this border is marked by a row of foramina which transmit vessels and which sometimes are of considerable size (fig. 33). These foramina can be seen, even before ankylosis, in rather more than 10 per cent. of the specimens examined. One large hole exists at the anterior superior angle in 20 per cent. of the cases examined (fig. 25). The lower end of the anterior margin extends downwards into the canal below the level of the hamulus in 80 per cent. of the bones, the upper end is confluent with the top of the crista in 8 per cent. This margin unites in a somewhat variable way with the nasal process of the maxilla, sometimes it forms with it a simple squamous suture, overlapping it, with the intervention of a thin layer of periosteum, sometimes it forms with it a harmonia, but in the largest number of cases examined (percentage not determined), the margin of the lachrymal widens and tends to split into two lamellæ, one passing in front, and the other behind the maxilla, forming thus an imperfect schindylesis. The lamella which passes internal to the maxilla is usually better marked above, that which lies external is better marked below; but I have not found a perfect schindylesis in any, as the two lamellæ have never been sufficiently developed to embrace the margin of the nasal process.

The inferior external margin, as seen in the orbit, is the most uniform and the least frequently ankylosed ($\frac{1}{4}$ per cent.); it always lies on a plane external to the superior, and passes from behind forwards and upwards; its posterior angle is nearly always rounded. This edge is sometimes a harmonia, but most commonly tends slightly to overlap the maxilla. Of its anterior termination we shall see the varieties when considering the hamulus. It is seldom irregular or lobulated, sometimes rectilinear and horizontal, but most commonly presents a single convexity.

Of all the margins the posterior is that which shows the greatest amount of lobulation, and it is sometimes deeply indented by bays, sometimes one or two of these extend almost to the crista lachrymalis; more rarely it sends a spur backwards into the ethmoid; this I have seen in three instances. In another instance a long process of the os planum of the ethmoid projected forwards below the lachrymal bone and below the inferior lachrymo-ethmoidal ossicle, separating these from the maxilla as far as the crista lachrymalis.

IV. The varieties of crista lachrymalis and of hamulus are very numerous, but may be reduced to four types—

A. Simple lamellar forms with obsolete crest.

B. Simple carinate forms.

C. Carino-acuminate forms.

D. Carino-hamate forms.

A. In the normal lachrymal the two lamellæ, orbital and lachrymal, which unite at the crista, form an angle which is not far from a right angle, but in those of this first variety the two parts are so nearly on the same plane that the crest is obsolete. This condition is rare, I found it only in twelve bones, mostly of old persons. There is no prolongation of the bone into the nasal canal, and the lower edge of the bone ends opposite the confluence of the nasal duct and lachrymal sac, that is, opposite the lower valve of Béraud ("Gaz. Médicale," 1851). Coexisting with this I have never found any separate ossicle nor any imperfect suture, and five of them at least were fenestrated with absorption holes.

B. The second or simply carinate forms are commoner, and vary in detail of development from those in which the orbital face of the bone shows a slightly ridged convexity below to those in whom the surface is marked by a strongly projecting ridge at the confluence of the grooved and the posterior lamella. In ten out of thirty-six such bones the crest was continuous from above downwards, in twelve it was rounded for the upper third, in nine for the upper half, while in five it was quite obsolete above. Of those in which it was continuous three had a well-marked spur of the crest in the upper part of its middle third, the surface behind which I have found giving attachment to the upper ascending fibres of the *musculus lachrymalis posterior* of Henke ("Archiv für Ophthalmologie," iv, 270). In none of these was the crest prolonged into a point below, but it ended by its lower border coming into contact with the *crista lachrymalis* of the maxilla. This form seldom coexists with anomalous suturation, was not uncommonly fenestrated or foraminated, sometimes ankylosed, and the edges when free were generally uneven.

C. The third form is the commonest among European skulls. I have found it in over 50 per cent. of British lachrymals, and in 42 per cent. of all examined. In most of these bones the crest becomes gradually more acutely elevated as it descends, and ends in a sharp point which overlies the edge of the maxilla. The lower edge of the bone close to the hamulus splits into two lamellæ, an outer which becomes continuously articulated with the orbital floor of the maxilla, an inner continuous downwards into the *processus turbinalis*, and forward into the *processus lachrymalis*, the latter of which usually comes in contact with the upper and outer edge of the *processus hamatus minor*, and the anterior edge of the unciform process of the ethmoid. The surface between these lies on the maxilla, but is sometimes, by the absorption of the intervening lamella of bone, brought in contact with the mucous lining of the opening of the *antrum maxillare*.

In rather more than 12 per cent. the apex of the crest where it touches the maxilla points directly to the inner end of the *sutura*

infraorbitalis transversa, and not uncommonly a *sutura verticalis* passes downwards and forwards therefrom, either to the infra-orbital or the accessory infra-orbital canal.

The crest in a few instances projects as a sharp thin lamella, and this has its free border either uniform or prolonged into spinous points. The commonest place for such prolongation is at the junction of the upper and middle third, a second often exists at the lower third, and these two may coexist. These spines are still more frequent in the bones showing the next variety.

D. The carino-hamate form is nearly as common as the last, and in the series of American crania in the Cambridge Museum it is found in three out of each five. Among European crania I have found it only in 27 per cent., and in 39 per cent. of the entire number examined. These vary from forms with a small acuminate spur—that is, strongly pronounced cases of the last form—to those with a large quadrate process projecting over the orbital ridge to the face; this last condition occurred in $\frac{1}{60}$ of all the European crania examined, but in $\frac{1}{22}$ of all crania. This is the form which is most commonly associated with accessory ossicles and sutures. In these cases, as well as in the last series, the *crista lachrymalis* frequently begins above as a rounded tuberous knob below the frontal suture, sometimes as a sharp ridge, and this may be curved forward so as to arch sharply over the lachrymal sac. This occurs in 2 per cent. In old bones this region is usually thinned and rounded, owing to the distension of the upper lachrymo-ethmoidal cell. Below this the crest at its upper third is often prolonged into a spine for the attachment of the *lachrymalis posterior* (attached not to the sharp edge, but to the posterior and outer surface of the spine), below which the crest is less prominent and is indented into a bay, generally again becoming prominent and merging into the hamulus. This arrangement of the crest exists in one-sixth of the bones examined.

In 2 per cent. of hamate bones the border of the crista is reduced to a thread 1 millim. in breadth; but I have not found this form to be constant among negroes, as Sömmering regarded it to be (Weber-Hildebrand, II, 401).

In three cases I found examples of the second spine above the hamulus at the lower third of the crista, similar to that described by Schwegel ("Henle u. Pfeufer's Zeitschrift," III Reihe, V, 1856, p. 866).

In five cases there was a perforation through the root of the hamulus, traversed in one instance by a branch of the infra-orbital artery. The hamulus is pierced by one or two holes in 2 per cent. of specimens, and these transmit similar vessels.

In one Malay skull the hamulus was wide and deep, reminding one of the condition in many catarrhine monkeys.

Of other points of superficial anatomy in connexion with this bone, the following may be noticed. While, in general, the orbital surface is uniform, I have several times found it to be irregular, probably as the result of accident. Usually this takes the form of irregular surface depressions. It may also present vascular grooves for branches of the infra-orbital artery, but these are not common.

The inner surface below presents two lamellar processes :—

1. *Processus lachrymalis*, the very variable concave lamella which projects into the nasal canal; usually this ends below in a rounded border, but in a few it ends in an acute down-directed point. The length of this rarely exceeds 3 millims.; the maximum which I have found is 6 millims.

2. The *processus turbinalis*, or the inferior internal margin, is by far the most irregular portion of the bone; it touches the inner side of the maxilla in front of the opening of the antrum, and generally is prolonged downwards to a point where it converges with the *processus lachrymalis*.

The lower margin of this articulates with the *processus lachrymalis* of the maxillo-turbinal in about 66 per cent. of cases. The forms of this articulation are variable; but in 70 per cent. the outer surface of the turbinal overlaps a small space of the inside of the process of the lachrymal.

The outline of this *processus turbinalis* is rudely triangular in about 70 per cent. with three sharp margins; in some there are four edges, an additional one being placed between the lachrymal process and the point of the turbinal process.

V. In size and degree of development there is little difference between the bones of the two sexes; the average male lachrymal has a maximum length in the orbit of 16·5 millims. (not counting the downward extension of the *processus lachrymalis*), the average female has a length of 15·5 millims. The shortest normal adult bone measured 8 millims., the longest 19·5. The breadth at the lowest border was more variable, the average being 9·5, the extremes 4 and 14. The lachrymal index $\frac{b \times 100}{l}$ averaged 60, but many varied between 42 and 75, the average female index being a little higher than the male. The width of the upper edge is the most variable, the widest of 70 measurements was 11 millims., the narrowest 2, the average 7·75.

The hamulus is commoner in male than in female skulls.

VI. The vascular relations of the lachrymal bone are usually very definite, a branch of the infra-orbital passes under, through, or over the hamulus into the lachrymal canal; branches of the infra-orbital and of the angular artery pierce the suture between the maxilla and lachrymal anteriorly and a branch of the nasal division of the ophthalmic artery passes through the suture at the fronto-maxillary angle

into the superior lachrymo-ethmoidal cell. In 20 per cent. there is a distinct aperture, sometimes of large size for this, and I have found in many cases a very fine twig of the infra-trochlear nerve to accompany the artery through this foramen, making thus, with the *nervulus spheno-ethmoidalis* of Luschka and the proper nasal, a third ethmoidal branch from the ophthalmic division of the fifth.

VII. The disposition of the periosteum on the outer surface of the lachrymal bone is worthy of note. Tracing it forward from the os planum on the orbital surface, it is without much difficulty divisible into two layers, and at the *crista lachrymalis* the innermost of these again divides into two lamellæ—one, which keeps close to the surface of the bone, lining the lachrymal groove, and is traceable forwards on the nasal process of the maxilla to the level of the lachrymal crest on that bone. This layer is separable from the outer wall of the lachrymal sac, to which it is attached by a loose connective tissue, in which many vessels ramify.* The outer layer of this inner lamella unites with the entire of the outer lamella and passes over the lachrymal sac as a lachrymal aponeurosis continuing forwards and inwards to unite with the former layer at the *crista lachrymalis anterior.* These layers underlie the *musculi lachrymales,* and are pierced by the *canaliculi,* along which, towards the puncta, they send an expansion, and beneath the inferior of which the deeper layer is strengthened by a more or less strong fibrous band, which starts from the lower edge of the crista lachrymalis and passes obliquely forwards to be attached to the outer and lower part of the crest of the same name on the maxilla. This is the band which, when ossified, becomes the *hamulus lachrymalis,* and which seems to be constantly present, either in this primitive membranous condition or else as a longer or shorter ossification in this membrane; the presence or absence of the hamulus is thus in reality the ossification or non-ossification of a constant though variable band. Beneath this band the small lachrymal branch of the infra-orbital artery passes to the nasal duct, and where it arches over this vessel, the membranous or bony hamulus not uncommonly gives attachment to fibres of the inferior oblique muscle, for the rest of which there is usually a distinct depression on the maxilla.

The outer connexions of the periosteal fibrous layer which lies superficial to the lachrymal sac are not easily traced; a transverse section through the inner canthus shows this layer to be continued forwards under the lachrymal caruncle into the ridge of fibrous tissue which gives origin to the *orbicularis palpebrarum,* and which, when detached from its surroundings, is called *tendo palpebrarum.* When

* This separation of the comparatively thin and simple mucosa along most of the course of the duct from the periosteum was described by Robin and Cadiat ("Journal de la Physiologie," 1875, p. 497).

simply cleaned and not detached, this shows, however, in its proper condition as a simple ridge of this lachrymal aponeurosis. Above and below the level of the *rictus palpebrarum* this lachrymal aponeurosis is continued outwards into the broad ligament of the eyelid, which is attached along the orbital margin, and with whose upper and inner border the trochlea of the superior oblique has originally been structurally continuous. From the anterior or facial side of this lachrymal aponeurosis the palpebral and ciliary parts of the *orbicularis palpebrarum* are attached, from the posterior or orbital side the *musculi lachrymales* (figs. 29, 30).

The grooved portion of the lachrymal bone is frequently traversed by very many fine holes. These are present even in very young states of the bone (fig. 1), and are even more distinct in the fourth month than in the bone of later age. Sometimes, however, this part of the bone remains quite cribriform (fig. 26), a condition never found in the orbital part, which may have a few vascular holes or absorption foramina, but which I have never found uniformly perforate.

The inner surface of the young lachrymal bone is covered with a very thin vascular periosteum, which separates it from the ethmoidal cartilage or bone. As age advances, this becomes with difficulty separable, and the underlying ethmoidal lamellæ become thin and finally become absorbed in parts; two lachrymo-ethmoidal cells, superior and inferior, become thus in the adult bounded by this bone externally, and the partition ridge of the ethmoid sometimes becomes ankylosed to the corresponding strengthened ridge of the lachrymal bone. In front of the superior ethmoidal cell, the lachrymal bone in about 60 per cent. is for a short space in contact with the narial mucous membrane, and in about the same proportion the posterior inferior angle forms the outer boundary of that part of the infundibulum into which the anterior ethmoidal cells open. The mucous membrane which touches the bone is inseparable from the periosteum, and is covered with ciliated epithelium. The superior lachrymo-ethmoidal cell opens into the infundibulum external to the middle spongy bone, and above and behind the opening of the antrum. The inferior opens lower down and farther forward into the same passage, close to the opening of the antrum. In old persons the superior lachrymo-ethmoidal cell extends backwards under the os planum.

The anterior margin of the uncinate process of the ethmoid articulated with the hollow of the crista in about 72 per cent. of the specimens.

VIII. Six separate ossicles occur around the margins of the lachrymal bone. These are—

1. Ossiculum ethmo-lachrymale superius.
2. Ossiculum ethmo-lachrymale inferius.
3. Ossiculum canalis naso-lachrymalis.

4. Ossiculum hamuli.
5. Ossiculum infra-orbitale.
6. Ossiculum maxillo-frontale.

Ossiculum ethmo-lachrymale superius (figs. 11, 23). In seven specimens I found this small ossicle behind the normal lachrymal in the upper part of the ethmo-lachrymal suture; in one of these, in a Kaffir, the ossicle occupied the entire space of the small ethmo-lachrymal suture, above a long ascending ethmo-lachrymal spine of the maxilla (fig. 23). In another specimen (fig. 11) the bone was large, and also completely separated the ethmoid from the lachrymal. This form is described by Krause (p. 69) as the *os lachrymale posterius*, and in most cases it is, as he has described, a dismemberment of the os planum. In one, however, I found this bone to overlie the ethmoid. A similar ossicle is described by Henle (p. 129). In the large sized forms it is really ethmoidal; in none did it extend back as far as the anterior ethmoidal foramen. In these forms it is a persistence of a commonly present separate centre of ossification in the front of the os planum (fig. 7).

Ossiculum ethmo-lachrymale inferius (figs. 11, 18) is a similar bone, not usually of ethmoidal origin, at the lowest part of the ethmo-lachrymal suture, between these bones and the maxilla. In one case only it was truly ethmoidal; in others it was supra-ethmoidal, and seemed to be a separated ossicle of the infra-orbital plate of the maxilla, which often (fig. 27) is irregularly cleft in this region. In fig. 11, it will be seen coexisting with a divided maxilla; and in fig. 23 a long cleft underlies the superior lachrymo-ethmoidal ossicle.

Ossiculum canalis naso-lachrymalis was so named by Gruber, and is a small bone lying external to the bony or membranous hamulus, and in front of the *sutura infra-orbitalis transversa* when such exists, usually coexistent with a *sutura verticalis*, at the commencement of which it is placed. This bone was first described by Béclard ("Mémoires sur l'Osteose, Nouvelles Journal de Médecin," IV, 1819, p. 332), and afterwards by Rousseau ("Annales des Sciences Naturelles," XVII, 1829, p. 86, Pl. V). Other anatomists have referred to it, but the extensive monograph on the subject by Professor Gruber ("Mém. de l'Acad. Imp. de St. Pétersbourg," VII Series, vol. 24, No. 3) is so full that to it little can be added. Professor Gruber regards the bone described by Béclard as distinct from the ossicle of Rousseau; but, while this may be, the descriptions given by these authors are not sufficiently definite to enable us to decide. In general, this bone is quite distinct from the *hamulus*, and coexists with it in the majority of cases, and corresponds to a variable amount to the outer inferior edge of the lachrymal in one of my cases, completely excluding it from contact with the maxilla.

The frequency of occurrence of this bone has been differently

stated. Rousseau says five or six out of every ten; Gruber says in the majority of cases; Krause, on the other hand, says in 20 per cent. In my series, I find that it is present as a distinct element in 32 per cent., but that there are traces of its having been present in many others in which it has lost its separateness by ankylosis, so that, on the whole, there are traces of its existence in 55 per cent. of crania. In shape it is crescentic, trigonal, quadrate, or irregular; in length it varies from 0·5 millim. to 7 millims. When the hamulus exists, the ossicle is excluded from the lachrymal canal by it, and I have never found it stretching beneath the hamulus so as to come into the wall of the tube. In cases of unossified hamulus, it usually forms part of the bony margin of the canal, dipping into it for not more than 1·5 millims., and underlying the lachrymal branch of the infra-orbital artery. This ossicle always lies on the maxilla and indents it, and not the lachrymal; and when it ankyloses, it is usually with the maxilla, not with the lachrymal, unless in exceptional cases. In nature it seems to be, like the inferior ethmo-lachrymal ossicle, a detached ossicle of the maxilla, one of several ossific nuclei of the orbital plate which has not coalesced with its neighbours.

The *ossiculum hamuli* is a separate ossification in the membranous hamulus (figs. 9, 13, 14, 15, 17, 21, 26) at the anterior attachment of the membrane, that is, at the front border of the lachrymal groove, separated by a vacant space or suture from the crista lachrymalis. In this form I have found it in $1\frac{1}{2}$ per cent. Sometimes it is large, extends backwards and replaces the hamulus, and it may coexist with the last ossicle and exclude it from the margin. In some cases, one ossicle of large size seems to replace the two (fig. 14). This bone is always at the margin of the canal, and never coexists with a developed hamulus; in six specimens it encroached on the face, and formed the *pars facialis* (fig. 13).

The *ossiculum infraorbitale marginale* (Gruber) is a small nodule in front of the malar, and belonging to it rather than to the lachrymal. I have found it to articulate with the lachrymal hamulus, with the *ossiculum canalis*, or with the *ossiculum hamuli*, but I reserve the descriptions of these as they really belong to the malar bone. When ossified to the malar, and extended to the lachrymal, they produce the condition of lachrymo-jugal suture (Gruber, *loc. cit.*, "Proc. Royal Irish Academy," Nov. 1874, p. 58).

Ossiculum maxillo-frontale is a bone present in 1 per cent. of my specimens, and formed by a detached slip of the maxilla along the upper part of the *crista lachrymalis anterior*. This is the *nebenthränenbein* of Luschka ("Müller's Archiv," 1858, p. 304) which Krause finds to exist in 3 per cent., a degree of frequency not present in my specimens. It is variable in size, usually an elongated triangle with its apex downwards articulating above with the frontal, posteriorly

with the lachrymal, and anteriorly with the nasal process of the maxilla. In others, it has partially ankylosed above and remains free below, its suture being in the plane of the *sutura imperfecta* of M. J. Weber. This bone was first described by Rosenmüller (" *Descr. partium ext. oculi*," Leipzig, 1797, p. 14), who found two instances. Budge (" Henle u. Pfeufer's Zeitschrift," III, Series VII, p. 278) found it 6 times in 184, and Luschka found it 7 times out of 60. Combining these with my 10 out of 1020 sets of bones, we get an average frequency of 23 in 1264, that is about 1 in 55. Taking into consideration its relation to the *sutura notha*, it seems probable that this must represent some element which tends to remain separate.

In my instances, the detached process of bone was never wholly coextensive with the part isolated in other skulls by the *sutura notha*, but it generally corresponded to it above. In the skull of Glorvina (Lady Morgan's Wild Irish Girl) the ossicle on the right is completely detached, that on the left, where the bone was much larger, has become ankylosed to the maxilla for a little more than the lower half of its extent, and this arrangement I have also found in three other instances.

The *sutura notha*, or *sutura imperfecta*, which is related to this ossicle, is remarkably constant in some form or other, usually as a row of holes into which branches of the infra-orbital artery pass (fig. 2). Traces of its separateness sometimes continue on the inside of the maxilla even when there is little sign on the outside, and not uncommonly a fissure occurs traversing the whole thickness of the bone at one extremity, usually below but occasionally above. It is, however, true that in the very large majority of maxillæ no separate continuous centre of ossification can be isolated for this region, but this part of the bone ossifies continuously from the anterior centre of the bone which arises below and internal to the infra-orbital hole in the membrane of the face, during the latter days of the fifth week of fœtal life. Thus if this suture marks out an element originally separate, it must have lost its distinctness at a remote period. The vessels perforating the maxilla along this line pass to the ethmo-maxillary and maxillo-turbinal sutures, and end on the nasal mucous surface along these lines; indeed, in general, this suture marks the distinctness of that part of the nasal process of the maxilla which overlies the anterior border of the ethmoid from that part which intervenes between the ethmoid and the nasal, and the ossicle last described corresponds to the upper part of the former region.

IX. The history of the development of the human lachrymal bone is interesting as showing something of the limits within which ossification periods vary. I have not been able to detect any trace of bone until the eighth week. In one fœtus of 4·25 centims., presumably

eight weeks old, I found a bony centre on the right side, in front of the nasal plate of the maxilla, but none on the left. In a second of the same size, both had begun to ossify, while in a ninth week fœtus I found no trace of bony growth. In nine other fœtuses of eighth week age, I found the traces of beginning ossification on both sides in the form of single bony centres, which begin in the membrane overlying the ethmoidal cartilage, and lying at the back of the lachrymal duct, which has a distinct lumen by this time.

At or near the end of the third month in fœtuses of 6–7 centims., the portion contiguous to the orbital plate of the maxilla is ossified, but does not touch any of the developing bones around, except behind and below the lachrymal grooves. In fœtuses of the end of the fourth month, the lower border is fully ossified, and forms a suture with the maxilla; ossification has also extended rapidly upwards, so that the upper margin touches the frontal. The crista is developed, and the orbital surface behind it is narrow, overlying the orbital face of the ethmoidal cartilage, in which the first bony nodule does not appear until the beginning of the thirteenth week. At this period, the lachrymal bone is somewhat trigonal, and measures 3·5 to 4 millims. long (average of eight), and in one of my specimens the hamulus has begun to ossify even at this age. Bony growth extends more rapidly upwards and forwards than backwards, and the lachrymal part of the bone is completed, except at the two ends, by the end of the fifth month, when as yet the orbital part is comparatively narrow. Even at this stage, the anterior part is generally much foraminated (fig. 1), and, while in contact with its other environing bones, it has not yet touched the bony *os planum* (figs. 6, 7), nor the maxillo-turbinal cartilage. The lowest part of the former is reached at the end of the fifth or very early in the sixth month, when the bone attains a length of 4·5 millims. In the sixth month, it has extended further back, and has increased in length to 5 millims.; in the seventh, it has attained nearly its adult shape, and it averages 6 by 3; in the eighth, all the margining sutures are established, except the lachrymo-ethmoidal, where there is still for the upper two-thirds an area of uncovered cartilage, and also for a short extent anteriorly and superiorly. The bone is now 6·25 by 4 millims., has attained its adult shape, and in half my specimens has a developed hamulus. In the ninth month, the average size is $7·5 \times 5$ millims., but there is yet an angle of cartilage above and behind unfilled. The hamulus is often fully ossified even to its facial part; indeed, I have seen this state attained in one fœtal skull of eight months.

The *ossiculum canalis naso-lachrymalis* is early in its appearance. In one eighth month skull I found it distinctly ossified, and in six skulls of ninth month fœtuses. The ossiculum infraorbitale I once saw in a seven months skull.

The contiguous part of the maxilla shows some noteworthy characters in development, scattered bony nuclei appear in the infra-orbital membrane and in the membranous nasal process during the early part of the sixth week, and in skulls of fœtuses of 32 millims. long these have coalesced into continuous plates, one behind and below the lachrymal in the floor of the orbit, the other in front and internal, and early continuous with the ossification of the front of the maxilla. The latter arises by several nodules irregularly divided into two sets, one of which forms the *crista lachrymalis anterior*, the others coalesce to form the rest of the nasal process, but these early unite, and in thirty fœtal skulls I have only once seen a distinct trace of a persistent suture between these elements after the end of the third month. There always, however, persists the row of vascular holes along this line becoming the *sutura notha* of the adult. The infra-orbital nucleus consists at its first appearance of several irregularly elongated bony spicules beneath the infra-orbital nerve. Very rapidly, however, as these coalesce and extend forwards towards the brim of the orbit the bone rises above the nerve close to the orbital edge and forms a bony bridge over it. There is always a continuous layer of membrane over the nerve, thicker at the margin even before ossification is detectible, traceable outward to the place where the malar is ossifying and onwards to the lachrymal region, and into this the bony growth extends. The overlap is from without inwards, and the lip of bone growing over the nerve touches the continuous plate internally forming the *sutura infraorbitalis verticalis* on the face. This has formed and the infra-orbital foramen is quite bridged in at the end of the thirteenth week. About the sixteenth week the margin of the outer lip of the infra-orbital canal extends over the nerve behind the brim of the orbit, and thus coalescing with the inner margin from before backwards forms the *sutura infraorbitalis longitudinalis*. The point where these two sutures unite varies in place and may be at, under, or internal to the anterior point of the malar bone, and, as in many cases, the part of the maxilla on which the malar abuts, is prolonged inwards to support the anterior inferior angle of that bone, it is not uncommon to find the front portion of the sutura infraorbitalis running transversely inwards into the lachrymo-maxillary suture, in this form the transversely-placed, inward-running anterior limb of the suture is sometimes known as the *sutura infraorbitalis transversa* (Halbertsma), and when this is present the edge of the portion of the infra-orbital plate internal to the infra-orbital nerve is thereby excluded from the brim of the orbit. The *ossiculum canalis* is the terminal nodule of ossification in the in-jutting extremity of the process from without.

The early period at which these conditions appear is interesting, and the extent to which the malar encroaches on the maxilla along

the orbital border is as variable in the twelve fœtal crania under four months which I have now before me as in the same number of adult skulls.

Of the later changes taking place in the development of the lachrymal bone the principal is the assumption of a more vertical position. The embryonic lachrymal until the end of the eighth month is inclined inwards at an angle of from 60° to 70°, but after birth it rapidly becomes more vertical and the surface develops its outward concavity from above downwards, the first appearance of which immediately precedes birth. The degree of obliquity of the fœtal lachrymal at birth may be represented by an inter-lachrymal index, that is, the maximum distance between the upper margins of the two lachrymal bones multiplied by 100, divided by the maximum distance between the lowest margin of the two bones. This in ninth month skulls averages 72.

X. In the adult the position and obliquity of the lachrymal bone depends on several factors, the size of the orbits, the internasal width, the width of the respiratory region of the nose, the degree of projection of the frontal bone, all influence it. The curve of the surface is not uniform, the lower third being generally turned rather sharply outward. The simplest method of estimating both the obliquity and curvature of the surfaces is by comparing the relative distances between the respective points of the two lachrymal bones. The average superior inter-lachrymal width or distance between the posterior superior angles of the two lachrymals is 25·2 millims. The posterior inferior inter-lachrymal width or distance between the posterior inferior angles, measured from the point of contact of lachrymal, ethmoid, and maxilla on each side is 30 millims., and the anterior inferior inter-lachrymal width, or that between the most divergent points of the two lachrymals, the extreme inferior point of the crista (not taking the hamulus into account), is 33·4 millims. The amount of inclination may be expressed by two indexes which I may call the anterior and posterior inter-lachrymal indexes. The latter of these (superior inter-lachrymal width $\times 100$, divided by posterior inferior inter-lachrymal width), indicates the degree of obliquity of the upper two thirds of the bone. The former $\left(\frac{s.i.w \times 100}{a.i.i.w}\right)$ indicates the obliquity plus the curve of the lower third, the amount of which can be estimated by comparison of the two indexes. The average anterior inter-lachrymal index $=75$, the average posterior $=84$. The former index decreases with increasing splay of the lower margin, the latter with increasing general obliquity.

These degrees of obliquity seem to vary in different races. In the crania of negroes from South-West Africa the bone is most vertical;

in the distorted crania of Peruvians it is most oblique. The following table exhibits concisely these divergences:—

Race.	Averages of measurement.				
	s.i.w.	*p.i.i.w.*	*a.i.i.w.*	*p.i.* index.	*a.i.* index.
	millims.	millims.	millims.		
3 Chinese	25·3	27·6	31·6	88	80
16 Negroes	27·7	31·6	34·2	87	81
8 Australians	25·3	29·3	32·4	86	78
50 Europeans	24·8	28·8	32·8	86	75·6
25 Peruvians	24	29·8	34	80	70

Comparing these it will be seen that the average internasal width of the negroes raises their indexes, while the outward splay of the lower third is greatest among the Peruvians. The bone is also generally smaller in negroes than in the other races.

XI. Comparative Anatomy and Morphology. The lachrymal bone is a splint developed in the membrane overlying the outer side of the ethmoid, and in mammals it is formed in close relation to the nasal duct. I have not found, in any human embryos, specimens young enough to show the earliest stages of the formation of the duct, but my examinations of sections of this region in rats and pigs confirms the description of the development of the duct given by Born and Legal ("Morph. Jahrbuch," viii, 1882, p. 353), according to whom it arises, not as Coste believed, by the closure of the surface over the persistent oculo-nasal groove, but by the in-projection of a column of epithelial cells from the surface in the line between the eye and the nose, the column becoming afterwards bridged by mesoblast, and developing a lumen. This agrees with the mode of development observed among the lower Vertebrates by Born. Before any trace of ossification appears in man the duct has a distinct lumen.

The formation of a double nasal duct is thus easily explicable, as produced by the interjection of a vertical fold of mesoblast into the column of cells. In the one instance of this very rare condition met with by me in the dissecting room, and now in the Cambridge Museum, there was a septum in the lachrymal sac, not quite dividing it into two, nor stretching above the level of the vestibule, this was continued into the nasal duct, completely dividing it for its upper fourth, each surface being clothed with ciliated epithelium. The septum ceased about the middle of the duct, ending obliquely, and from this to the inferior opening the tube was single, but its lower

end was crossed by a bridge of mucous membrane subdividing it into two.

In man, ossification commences in the membrane behind and internal to the lachrymal sac and duct independently of the ossification in the bridge of tissue overlying the tube. The former gives rise to the lachrymal bone, the latter to the nasal process of the maxilla.

The development of the bony environment of the sac is, however, variable among mammals, and three distinct types are met with: 1st, that in which the pre- and post-lachrymal ossification are continuous, and the bone forms a complete girdle for the nasal duct and a large element of the interorbital region of the face; 2nd, that in which, while still engirdling the lachrymal sac, the bone is reduced to a small thin lamella; and 3rdly, the form in which the lachrymal bone is confined to the post-lachrymal region.

The first form is met with among the lower and more generalised mammals. In all the Didelphia it is thus large, and has a wide lachrymo-jugal suture, larger in the carnivorous than in the herbivorous forms, but in none excluding the maxilla from the frontal.

Among the Ungulates the bone is large, with a large facial region. In many it has a large lachrymo-nasal suture, and excludes the maxilla from the frontal superficially. This is the case in Rhinoceros, Hippopotamus, many pigs, horse, Bos, Oreas, Tragelaphus, and many Artiodactyles, but there is a maxillo-frontal suture in Hyrax, some pigs, camels, Hyæmoschus, &c. In the pig there are often accessory lachrymal ossicles in the lachrymo-frontal suture.

Among all these the lachrymal tends to ankylose with the frontal and malar, not with the maxilla. The same large form exists through the Edentata and Sirenia, and a smaller form of the same kind among the Rodents. The chief variation among these last is the presence or absence of a jugal contact. There is a lachrymo-jugal suture among the Sciuromorphs and Myomorphs, and a few of the Hystricomorphs, such as Lagostomus, but it is generally absent among the other porcupine allies, owing to the shortening of the malar, not to any great alteration in the lachrymal, which in most Rodents has a larger facial than orbital surface, though not much of either. The Elephant, rodent-like in all its features, agrees with the Hystricomorphs in this.

Among Carnivores the lachrymal is small, forms but a small inconspicuous element of the orbital margin, and from its condition of moderate size among Cynoids diminishes both towards Œluroids and Arctoids, but especially towards the latter, among whom it appears as a very small lamina. It becomes still smaller among Pinnipeds, appearing in some like Callocephalus and Halichœrus as an exceedingly slender, often scarcely ossified, detached scales, which is lost in

most macerated skulls, and seems to be quite absent in Otaria and Cystophora.

The Prosimii preserve the same type of bone, but in adult skulls its early ankylosis with the maxilla renders it often difficult to isolate. They have all well-marked lachrymo-jugal sutures with lachrymo-maxillary sutures both in front and behind the malar. Chiromys has a large premaxillo-lachrymal suture, but this I have not noted in any other Lemuroid.

Among the Primates, the two types of smaller lachrymal exist. It is large, facial, and articulates with the nasal in Mycetes; smaller facial, and perforate in Ateles with a wide outreaching hamulus, whose lower surface partly roofs in the infra-orbital canal. In Cebus Pithecia, Callithrix, and Nyctipithecus it is imperforate, but has a very large hamulus, and the same form exists among the Arctopithecus. In Callithrix, from the peculiar construction and absorption of the interorbital septum, the back of the small lachrymal is bounded by a fronto-maxillary suture, and the same exists in Nyctipithecus.

The Catarrhines present several varieties of form; the perforate form with the continuous anterior margin is present in Cercopithecus and Macacus, while the anterior bar is medially interrupted in Semnopithecus and Nasalis. Macacus nemestrinus alone has a lachrymo-jugal suture, among the Catarrhines. The hamulus in all the cynoid Catarrhines is very deep and wide, extending usually to the face and spreading outwards far into the floor of the orbit.

The Gorilla has a long upper angle ascending forward to the frontal, and a short ethmoidal suture. As in all the other Catarrhines, the lower end of the lachrymal process projects, shelf-like, forwards. In five skulls I found a sutura notha and a sutura infraorbitalis verticalis.

The Orang has a long sharp hamulus, so placed that its edges look out and in, its surfaces up and down. The Chimpanzee has also a small lachrymal with a wide hamulus.

By following the lachrymal bone downwards among Vertebrates, something of the stages whereby it has become specialised from the inner elements of the suborbital ring of dermal ossicles, can be traced.

Among birds the bone occupies a position comparable with that in mammals, is exceedingly variable in form, not pierced by the lachrymal duct, and often attended with accessory ossicles, one (falcons, &c.), two (puffins, &c.), three (Procellaria, &c.), or four (Psophia).

Among Reptiles the same inner region of the orbit is occupied by two dermal ossicles, membrane-prefrontal and lachrymal, and occasionally by small granular accessory ossicles as well. Among the Amphibians, in the skulls of some of the extinct Labyrinthodonts, such

as Trematosaurus, there are bones in this region probably of the same nature, and comparing these with the suborbital ring in teleost fishes, such as the salmon, the innermost of the series seems so plainly to represent the lachrymal that Professor Parker has affixed this name to it in his figures (fig. 21, "Morphology of the Skull").

The lachrymal bone has been inherited by the mammalia in its large facial form, and it participates with its neighbouring bones in the fixity correlated with the setting apart of the jaws for mastication. The upper jaw must be fixed to give attachment to the masseter, which in most of the lower mammals exceeds the temporals (in most Ungulates $m : t :: 1\frac{1}{4}$ *to* $2\frac{1}{2} : 1$); accordingly the infra-orbital ridge is consolidated into a reversed arch with two piers attached to the skull, of the inner of which the lachrymal forms a solid integral part, and all these marginal elements tend to ankylose into a continuous bony ring.

While these conditions persist the bones retain the same relative place, and we can correlate the changes in structure with changes in function. Among the Primates the approximation to parallelism of the orbital axes diminishes largely the extent of all the elements of the interorbital septum and is correlated with a strengthened rim of the orbit, especially with an increase in strength in the fronto-jugal pier of the suborbital arch, which now bears the entire masseteric strain. The increase in size of the cranial cavity, together with the diminished interorbital width causes the duct to become entorbital, and it descends obliquely downwards and outwards in the lower forms, nearly vertically in man; the degree of obliquity depending on the width of the inferior nasal meatus and the narrowness of the interorbital septum. When the duct is obliquely placed the hamulus is large and bony to bear off pressure; when vertical the hamulus diminishes. While the duct is thus entorbitally displaced, the plane of the teeth is almost completely preorbital, and the pressure of the teeth is borne by the frontal process of the maxilla in front of the orbit, while the masseteric tension is borne by malar bone. Owing to the operation of these three factors, the lachrymal bone sinks into mechanical insignificance. That it is a diminishing bone in man is shown by the early stage at which it attains to its adult condition, as well as by its great variability.

The Ungulate lachrymal bears a tubercle, which is prominent in the Rhinoceros and Hyrax, and which either lies internal to or separates the canals. This corresponds to the projection of the periosteum, which in man forms the *tendo oculi*, and from it a continuous ridge of periosteum is continued inwards and forwards, from which in Hippopotamus, Rhinoceros, and Ruminants the orbicularis palpebrarum arises. The small accessory ossicle in the pig is late in ossifying, only showing its first trace of bone after birth.

Professor Gegenbaur, in his valuable paper on the *pars facialis* of the lachrymal bone ("Morph. Jahrbuch," 1882, p. 173), has pointed out that the hamulus is the passage between the pars facialis and the pars orbitalis. The former, present as a definite element in 4·25 per cent. of orbits is, as he has indicated, an atavism.

In man the nasal process of the maxilla very early projects in front of the anterior border of the ethmoidal lateral mass, and thus encroaches on the territory which in earlier conditions was only occupied by the lachrymal. The ossification, extending upwards, stretches on both sides of those branches of the infra-orbital artery, which turn in to terminate on the mucosa clothing the front of the ethmoid, making thus the line of vessels or *sutura notha.*

In the development of the large lachrymal bone of the pig ossification springs from a common centre, from which it extends around the duct. In man, in those cases wherein an ossiculum hamuli exists, there is really a second ossific centre in the bone, and in those instances in which an ossiculum maxillo-frontale exists, this bony segment of that part of the nasal process of the maxilla overlying the ethmoid has had its special centre. All these facts are instructive as showing the comparatively low morphological value of mere centres of ossification.

EXPLANATIONS OF THE PLATES.

The following letters refer to most of the figures.

e = os planum of ethmoid.
e. i. = ossiculum ethmo-lachrymale inferior.
e. s. = ossiculum ethmo-lachrymale superior.
f. = orbital plate of the frontal.
f. v. = vascular foramen.
h. = hamulus.
l. = lachrymal bone.
lac. = lacuna.
m. = malar.
m.-l.-s. = maxillo-lachrymal suture.
mx. = maxilla.
o. c. = ossiculum canalis.
o. h. = ossiculum hamuli.
o. m. f. = ossiculum maxillo-frontale.
s. i. t. = sutura infraorbitalis transversa.
s. i. v. = sutura infraorbitalis verticalis.
s. n. = sutura notha.

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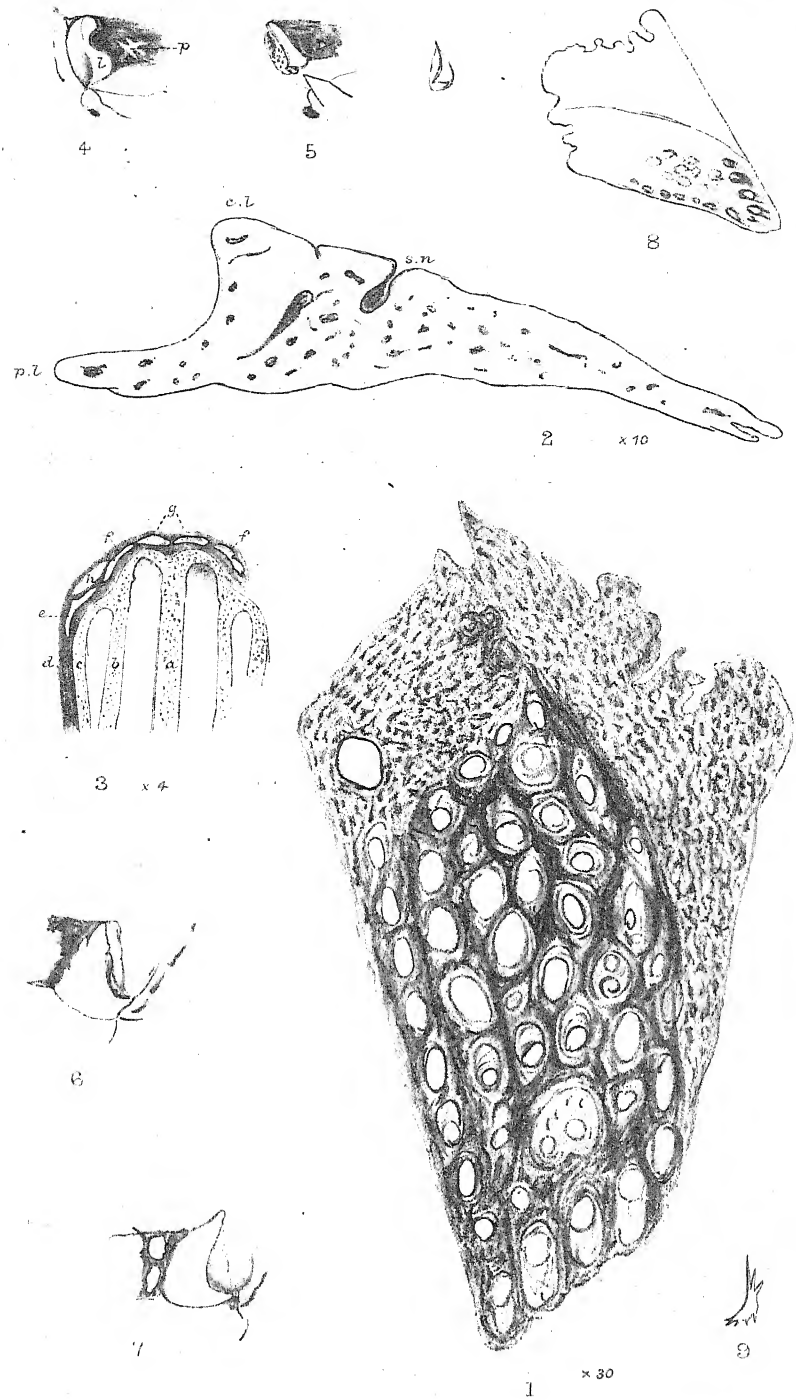
1. Orbital surface of lachrymal bone of fœtus, middle of fifth month × 30.

2. Transverse section of nasal process of right maxilla of girl, aged about 24. *p. l.* = processus lachrymalis; *c. l.* = crista lachrymalis × 10.

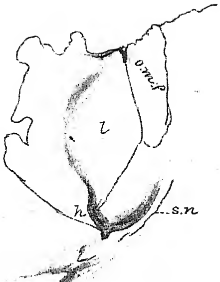
3. Transverse section of lachrymal region in fœtal skull of twelfth week. *a* = cartilaginous septum; *b* and *c* = ethmoidal cartilage; *d* = periosteum; *e* = section of

lachrymal bone; f = section of nasal process of maxilla; g = section of nasal bones; h = lumen of lachrymal sac.

4. Lachrymal region of fifth month fœtus.
5. *Ibid.*, fourth month.
6. *Ibid.*, seventh month.
7. *Ibid.*, eighth month.
9. Lachrymal bone of eleventh week, outline.
10. Lachrymal region of adult, with *ossiculum maxillo-frontale.*
11. *Ibid.*, with four ossicula coexisting.
12. Coexistence of *ossic. hamuli* and *o. canalis.*
13. *Ibid.*, with large *o. ethmo-lachrymale superior* with *o. hamuli* and *o. canalis.*
14. *Ibid.*, with very large *o. hamuli* encroaching on maxilla.
15. *Ibid.*, *o. hamuli* and *o. canalis* both separate from lachrymal.
16. *Ibid.*, *o. canalis* coexisting with hamulus and peculiar form of *sutura notha.*
17. *Ibid.*, *o. hamuli* with vascular hole between it and lachrymal.
18. *Ibid.*, with *o. ethmo-lachrymale inferior.*
19. *Ibid.*, *o. hamuli* ankylosed to nasal process of maxilla, *o. canalis*, to orbital plate.
20. Absent lachrymal bone, replaced by backward extension of nasal process of maxilla (x), between which and the upward extension of the inner portion of the orbital plate is the suture (y). There is an *ossiculum canalis* with a very small lacuna above it.
21. Well-marked *sutura notha*, small *o. hamuli* turned into the canal.
22. Double lower end of *sutura notha*, foraminated lachrymal groove and small *o. canalis.*
23. Small *o. ethmo-lachrymale superius* occupying the entire region of the ethmo-lachrymal suture; maxillo-lachrymal suture ankylosed.
24. Persistent vertical suture and *o. canalis.*
25. *O. maxillo-frontale.*
26. Arrangement of ossicles and sutures, with vascular foramina.
27. Inner surface of lachrymal bone, showing its relations to lachrymo-ethmoidal cells (*s. l. c.* and *i. l. c.*) and antrum (*a*). *i.* = infundibulum; *m. t.* = maxillo-turbinal.
28. Case of deficient lachrymal, with small lacuna and no ossicle.



West Newman & Co. lith.



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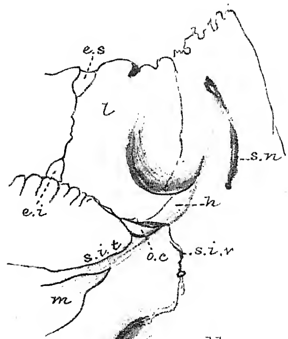

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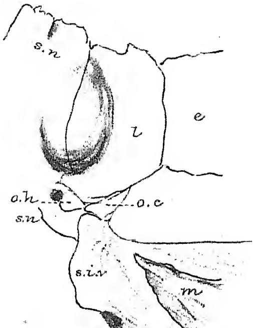

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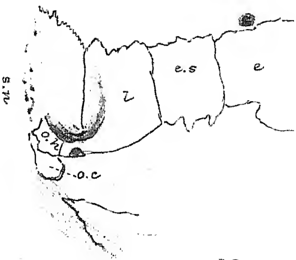

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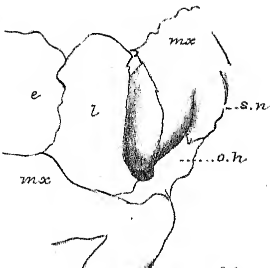

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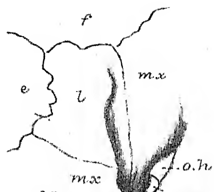

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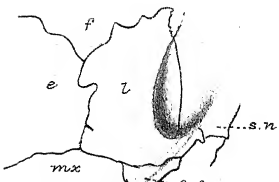

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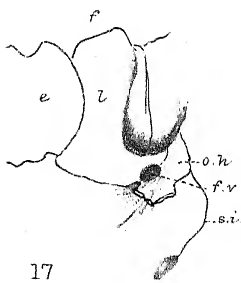

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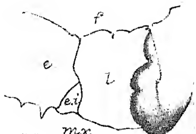

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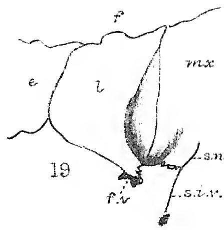

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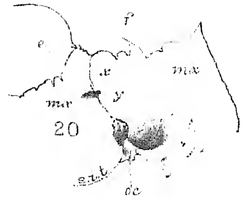

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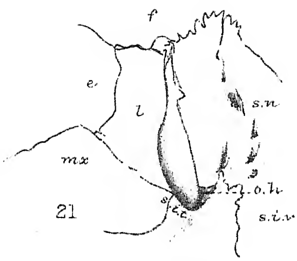

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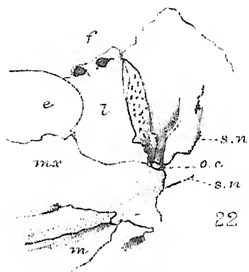

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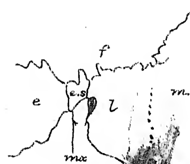

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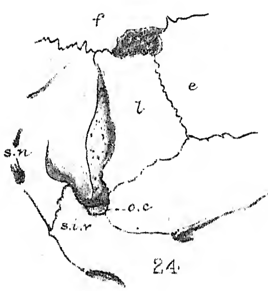

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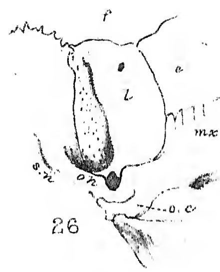

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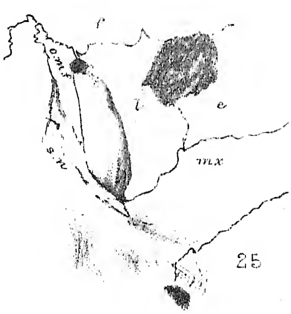

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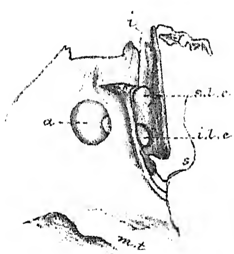

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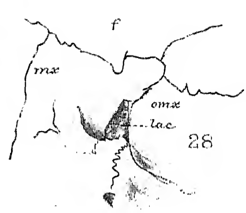

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West, Newman & Co. lith.

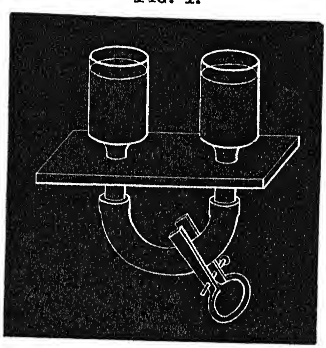
"Some Relations of Heat to Voltaic and Thermo-electric Action of Metals in Electrolytes." By G. GORE, LL.D., F.R.S.

Received November 14. Read November 22, 1883.

In several papers on the "Thermo-electric Properties of Liquids," &c., more particularly those in the "Philosophical Magazine," January, 1857; the "Proc. Roy. Soc.," No. 188 (1878), pp. 513–543; No. 199 (1879), and No. 208 (1880), I have shown the thermo-electric behaviour of numerous liquids with electrodes of platinum and mercury; and to a small extent with those of palladium, gold, silver, copper, and iron. In the present research, the subject has been examined in a different aspect, the object now being to ascertain the thermo-electric actions of a series of metals in particular liquids; and to examine the relations of the thermo-electric to the chemico-electric behaviour of metals in electrolytes, and to ordinary chemical corrosion, and the source of voltaic currents.

The following apparatus and method was employed for the thermo-electric experiments (see fig. 1). Two thin glass vessels of the form shown in the sketch, the cups of which were about 4 cm. wide and 5 cm. deep, with open legs 6 cm. long, and 13 mm. diameter, were

FIG. 1.



fixed in a piece of wood, and the limbs connected by an india-rubber tube. In using it to ascertain the thermo-electric behaviour of a particular metal with a liquid, the flexible tube was closed by a pinch tap, one limb and cup was nearly filled with the cold liquid, then the hot liquid (previously heated in a large closed flask) poured into the other; the pinch tap then taken off, the pieces of same metal wholly

immersed in the liquid, and then connected with the galvanometer. Thermometers were supported in the liquid of the two cups, and the temperatures of the two portions of liquid and pieces of metal were in every case (except otherwise stated) 60° and 160° F. respectively. All the metals and compounds employed were the purest I could obtain, and liquids were avoided which would yield a metallic deposit upon the metals. A series of metals and one liquid were examined at a time. The most frequently recurring difficulty in the research was polarisation. In all the cases the thermal, electrolytic, and polarisation effects, produced in the liquids at the electrodes by the passage of the current, influence and modify the results by secondary action. When also the current is sufficiently strong, the positive metal is corroded.

By these means the thermo-electric couples found with one liquid and a series of metals were first classed into two groups, viz., those in which the metal was thermo-electro-positive, and those in which it was negative. To determine the series from the two groups, two similar apparatuses, as already described, were simultaneously used, one containing two pieces of one of the metals A, and the other, two of another metal B, from the same thermo-electric group, and so connected in single series, that the two currents opposed each other, and the one alone of greatest E.M.F. deflected the needle of the galvanometer. The following table exhibits the results, and the position of each metal in order of electro-motive power to the one next below, and to that next above it in the series, was thus in every case experimentally ascertained. The top metal in each column is the most positive in the particular liquid; and the metals above the dividing lines are those from which the current proceeds from the hot piece through the liquid to the cold one, and the ones below those lines the reverse.

The proportions of substances dissolved in each ounce of water to form the solutions were as follows: of potassic cyanide (containing 93·52 per cent. of the actual substance), fluoride, bromide, nitrate, iodide, chloride, sulphate and hydrate, sodic diphosphate, chloride, sulphate, potash alum and ammonia alum, 10 grains each, potassic carbonate 12 grains, anhydrous magnesic sulphate 4·8 grains, oxalic acid 2 grains, dextro-tartaric acid 1 grain, chloric acid 6 minims, hydrochloric, formic (sp. gr. 1·18), sulphuric, and nitric acids $1\frac{1}{4}$ minims each. The liquids were selected of such a strength as was considered to be the most suitable, especially as regards rate of corrosion and strength of current. It was necessary in these experiments to continually remember that the results vary considerably with different specimens of the same metal, even when the metal is very pure and the pieces of it are cut from different parts of the same sheet.

Table I.—Thermo-electric Series of Metals in Liquids.

	1. KCy.	2. KF.	3. KBr.	4. KNO_3.	5. K_2CO_3.	6. Na_2HPO_4.	7. KI.	8. NaCl.
1.......	+ Sn	Al	Fe	Al	Al	Al	Sn	Pd
2.......	Al	Mg	Sn	Ni	Sn	Sn	Al	Cu
3.......	Pd	Sn	Ni	Fe	Pb	Cd	Fe	Sn
4.......	Ni	Fe	Co	Sn	Zn	Fe	Ni	Pb
5.......	Zn	Ag	Pb	Cd	Fe	Cu	Pd	Ni
6.......	Cd	Pt	Al	Cu	Cd	Pb	Pt	Fe
7.......	Fe	Au	Zn	Pb	Au	Ni	Au	Zn
8.......	Au	Pd	Mg	Pt	Cu	Pd	Zn	Cd
9.......	Pt	Pb	Pt	Au	Pt	Pt	Pb	Mg
10.......	Cu	Zn	Ag	Ag	Ag	Au	Cd	Al
11.......	Pb	Cd	Au	Pd	Pd	Ag	Cu	Pt
12.......	Ag	Ni	Pd	Zn	Ni	Mg	Ag	Ag
13.......	– Mg*	Cu	Cd	Mg‡	Mg	Zn	Mg	Au

	9. HCl.	10. KCl.	11. K_2SO_4.	12. Acid, oxalic.	13. Acid, formic.	14. Acid, dextro-tartaric.	15. KHO.	16. H_2SO_4.
1.......	Ni	Ni	Ni	Al	Al	Ni	Cd	Al
2.......	Fe	Zn	Sn	Ni	Ni	Mg	Fe	Ni
3.......	Al	Al	Fe	Cu	Fe	Al	Cu	Fe
4.......	Mg	Cd	Al	Mg	Sn	Fe	Ag	Mg
5.......	Cu	Pb	Pb	Sn	Mg	Sn	Zn†	Cu
6.......	Sn	Fe	Ag	Cd	Cu	Pb	Pt	Pb
7.......	Pb	Sn	Cu	Pb	Pt	Cu	Sn	Pd
8.......	Ag	Cu	Mg	Fe	Pd	Zn	Pb	Sn
9.......	Zn	Ag	Zn	Zn	Ag	Cd′	Ni	Cd
10.......	Pt	Au	Cd	Ag	Cd′	Au′	Al†	Zn
11.......	Pd	Mg	Pd	Pd	Pb	Ag	Pd	Au
12.......	Cd	Pd	Pt	Au	Zn	Pd	Au	Ag
13.......	Au	Pt	Au	Pt	Au	Pt	Mg	Pt

Table I—(continued).

	17. Na_2SO_4.	18. HNO_3.	19. $HClO_3$.	20. $MgSO_4$.	21. Potash alum.	22. Ammonia alum.
1.......	Ni	Al	Al	Ni	Al*	Ni
2.......	Fe	Ni	Mg	Al	Mg†	Sn
3.......	Sn	Mg§	Pb	Cd	Ni†	Fe
4.......	Cd	Cu	Zn	Fe	Sn	Zn
5.......	Pb	Fe	Cd	Sn	Fe	Al
6... ...	Au	Pb	Sn	Cu	Pb*	Mg
7.......	Cu	Ag	Cu	Ag	Cd	Pb
8.	Al	Zn	Ag	Zn	Cu	Cd
9.......	Zn	Cd	Ni	Pd	Pd	Cu
10.......	Ag	Sn	Fe	Pt†	Zn	Ag
11.......	Mg	Pd	Pt	Au	Au	Au
12.......	Pd	Pt	Au	Mg	Pt	Pt
13.......	Pt	Au	Pd	Pb	Ag	Pd

* Indicates a gradual reversal with *one* metal only.

† Indicates that a temporary reversal occurred when the currents from the two contiguous metals (*i.e.*, the one marked and the one *above* it) were opposed.

‡ Magnesium was first negative, then positive, then negative 38 degrees.

§ Magnesium was first positive, then negative, then positive 30 degrees.

Remarks.—Of the foregoing cases, only those in which the metals are not at all corroded, are entirely thermo-electric, the remainder are concrete ones involving the additional influences of ordinary chemical corrosion and of voltaic action, and this fact must be remembered.

The currents were feeble in solutions of hydrochloric, formic, and dextro-tartaric acids, notwithstanding that the corrosion of the more electro-positive metals appeared to be as strong as usual, and the currents were so weak with cadmium in formic, and cadmium and gold in tartaric acid, that the positions of those metals as given in columns 13 and 14 are uncertain. The direction of the currents obtained with aluminium, also with magnesium, in a solution of one grain of lævo-tartaric acid per ounce of water, was the same as in a solution of the same strength of ordinary or dextro-tartaric acid. By opposing to each other also the thermo-electric currents from those two solutions either with aluminium or magnesium, that from the one of the lævo-acid was in each case found to have slightly the greatest electric potential.

In some separate experiments, the strength of thermo-electric current from platinum at 60° and 160° F., in a solution of potassic cyanide, varied directly as the strength of the liquid, the deflection

of the galvanometer needle, with a solution containing ·3 grain of the salt per ounce of water being 1·0, with ·6 grain 2·0, 6·0 grains 8·25, and 12·0 grains 11·0. The hot platinum was positive. With mercury at those temperatures, in solutions of ·5 grain, 5·0 grains, and 10·0 grains of that salt per ounce of water, the hot metal was negative, and the deflections were 14·0, 45·0, and 50·0 respectively.*

The first point to be observed with regard to the table is, that out of 286 instances contained in it, including an average variety of liquids of alkaline, neutral, and acid reaction, in 87 or 30·42 per cent. only, or a proportion of 1 to 2·27, was the hot piece of metal negative to the cold one.

The thermo-electric order of metals in liquids was with every solution widely different from the ordinary thermo-electric order of metals alone, with no one liquid did more than three out of the thirteen metals agree in position in both the orders. It is remarkable that magnesium, a metal so very corrodible, so highly chemico-electro-positive, and so considerably thermo-electro-positive, in the series of metals only, is so often the most thermo-electro-negative in these series; in each case however where magnesium was at or near the bottom of the thermo-electric series, it was covered with a thin film of insoluble matter (see also pp. 261, 270, and 278).

The following list of the number of times each metal was positive and negative in Table I is inserted for future reference :—

Table II.

	Positive.	Negative.
Fe	21	1
Sn	20	2
Al	19	3
Ni	19	3
Pb	18	4
Cu	17	5
Zn	15	7
Cd	14	8
Ag	13	9
Mg	12	10
Pd	11	11
Pt	11	11
Au	9	13
	199	87
Proportion	2·287 to 1·0.	

* See "Proc. Roy. Soc.," No. 199, 1879, Experiments 1—4. Mercury was tried in a different apparatus.

With regard to the influence of composition of the liquid, the salts of simple chemical character, such as potassic cyanide, fluoride, bromide, and nitrate, gave the largest proportion of thermo-electro-positive couples; the more complex ones, such as the double sulphates, gave the largest number of negative ones. I have not examined whether this is related to differences of specific heat of the salts. Even the highly thermo-positive metal nickel was rendered thermo-negative in solutions of potassic hydrate and chloric acid, and similarly with the thermo-positive metals platinum and palladium, each in eleven different liquids, and with aluminium in solution of potassic hydrate, sodic sulphate, and ammonia alum. The usually thermo-negative metal iron was more frequently positive than any other metal, it was strongly so in solutions of potassic bromide, hydrochloric acid, caustic potash, and sodic sulphate, and so also was the thermo-negative metal cadmium in solution of caustic potash.

The liquids in which the hot metal was thermo-electro-positive in the largest proportion of cases were those containing highly electro-positive bases, such as the alkali metals; thus, the highly chemico-electro- and thermo-electro-positive body potassium, forms the highly thermo-electro-negative compounds potassic cyanide, fluoride, bromide, nitrate, carbonate, and iodide. From a much smaller number of facts, I had previously drawn the conclusion that, provided ordinary chemical action is absent, liquids of alkaline reaction are usually thermo-electro-negative, and acid ones, thermo-electro-positive. This is largely confirmed in the present table in the cases of the least corrodible metals, gold and platinum.

In every experiment of this kind, the two pieces of metal were simultaneously immersed in the hot and cold portions of liquid respectively, and no cases of reversal of direction of current therefore occurred; many would have happened had the metals been slowly heated in the liquids. In a subsequent series of similar experiments, in which the one limb of the vessel was gradually heated after immersion of the electrodes, a number of such instances were observed (see p. 281). Bleekrode also ("Poggendorff's Annalen," 1870, vol. cxxxviii, p. 571; "Annales de Chimie," April, 1870; also "Philosophical Magazine," 1870, vol. xl, p. 311) has recorded a few of such cases.

In order to obtain a thermo-electric series of all the strongest thermo-positive and negative couples in Table I, the electric potentials of most of the top and bottom couples were measured. The remaining members were omitted because their currents were very variable in potential, probably in consequence of polarisation, and of alterations of rapidity of diffusion of the layers of liquid in contact with the electrodes. The electric potential usually declines quickly (but occasionally only a little), hence the numbers obtained depend upon the degree of rapidity with which the determinations are made. The

time usually occupied varied from one to two minutes. All the measurements, except those of Tables XI and XIV (pp. 273 and 277), were made by *balancing* the current to be measured, by one of equal potential from two thermo-electric piles, each consisting of about 300 pairs of iron and German silver wires, the accuracy of which was occasionally tested by means of a Clark's standard cell.* (For a description of the pile, see "Proc. Birm. Phil. Soc.," vol. iv, pt. 1.)

With the two pieces of metal at 60° and 160° F. respectively, the following numbers and series were found, in which the top couple of the upper section is the most positive, and the bottom one of the lowest division is the most negative.

Table III.—Electric Potentials of Thermo-electric Couples.

Metal.	Liquid.	Volts.
Al	Na_2HPO_4	·6621
,,	Formic acid	·4392
,,	HNO_3	·4317
Ni	Tartaric acid	·3447
Al	$HClO_3$	·3051
,,	KF	·2914
,,	H_2SO_4	·2778
Ni	K_2SO_4	·2428
,,	Am alum	·1972
Sn	KCy	·1827
Ni	$MgSO_4$	·1757
Fe	KBr	·1702
Ni	KCl	·1439
,,	HCl	·0978
,,	Na_2SO_4	·0975
Cd	KHO	·0877
	Total	4·1475
	Average	·2592
Pb	$MgSO_4$	·0119
Au	HCl	·0140
,,	Formic acid	·0236
Mg	KI	·0282
Pt	Na_2SO_4	·0324
Pd	Am alum	·0441
Au	K_2SO_4	·0504
,,	HNO_3	·0660

* Nearly all the measurements of potential in this research were made with the aid of a Thomson's reflecting galvanometer, having a resistance of 3040·7 ohms.

Table III—(continued).

Metal.	Liquid.	Volts.
Mg	KCy	·0691
Pt	Tartaric acid	·0761
Ag	K alum	·0844
Pd	$HClO_3$	·0985
Mg	KHO	·1169
Pt	H_2SO_4	·1403
Mg	K_2CO_3	·1498
	Total	1·0057
	Average	·0670

Remarks.—The current from the strongest pair in this series had about one hundred times greater potential than that from a single pair of bismuth and antimony with an equal difference of temperature.

Not only were the thermo-electro-positive combinations of metals and electrolytes about double the number of the negative ones (see Table I): but the range of potential of the strongest of the former was about 4·12 times that of the latter, as shown by this table. The facts that there is a much larger number of thermo-electro-positive metals than of thermo-negative ones; also of thermo-electro-positive combinations of metals with liquids than of negative ones, and that the positive elements of each of these classes are usually the strongest, indicate that electro-positive action in metals generally is more frequently increased than decreased by rise of temperature.

In the present table, the acids in contact with the chemico-electro-negative metals platinum, gold, and palladium, are thermo-electro-positive; and in contact with the positive ones, aluminium and nickel, they appear negative. In this table also, as in Table I, aluminium, a strongly chemico-electro-positive metal, is conspicuously the most thermo-electro-positive one. The highly chemico-electro-negative metals also, silver, palladium, gold, and platinum, are present only in the thermo-negative division. In apparent opposition to this, but still largely in accordance with Table I, it is remarkable that magnesium, a metal highly chemico-electro-positive, and also considerably thermo-positive in the series of metals only, is the most thermo-negative in this series (see also pp. 255, 261, 270, and 278).

In order to ascertain the influence of strength of liquid upon the sequence of the various thermo-electric series, another set of experiments, similar to those of Table I, was made, with all the solutions of five times the strength, except those of sodic diphosphate, which was four times, and those of potassic and ammonic alum, and potassic sulphate, each of which was only three times. The results are shown in

the following table. The temperatures of the hot and cold pieces of metal were 60° and 160° F. respectively.

Table IV.—Thermo-electric Series of Metals in Strong Solutions.

	KCy.	KF.	KBr.	KNO_3.	K_2CO_3.	Na_2HPO_4.	KI.	NaCl.
1.	Sn	Ag	Ni	Ni	Sn	Sn	Sn	Ni
2.	Al	Sn	Fe	Al	Al	Al	Fe	Fe†
3.	Ni	Al	Sn	Fe	Zn*	Pb	Al*	Cu*
4.	Au	Fe	Cu	Cu	Cd†	Ni	Ni	Sn
5.	Pd	Pt	Pb	Sn	Pb*	Pd	Pt	Cd
6.	Pt*	Au	Ag	Pb	Fe	Pt	Pd	Pb
7.	Zn*	Pd	Zn*	Au	Cu	Au	Pb	Pd
8.	Fe	Ni*	Pt	Ag*	Au	Ag	Au	Ag
9.	Cu	Pb	Pd	Cd*†	Ag	Cu	Zn	Zn*
10.	Pb	Zn†	Au	Pt	Pt	Fe†	Cu	Mg
11.	Ag	Cd	Cd*	Pd	Pd	Cd	Ag	Al
12.	Cd	Cu†	Mg*	Mg*	Ni	Zn†	Mg	Au
13.	Mg*	Mg	Al*	Zn	Mg	Mg	Cd	Pt

	HCl.	KCl.	K_2SO_4.	Oxalic acid.	Formic acid.	Dextro-tartaric acid.	KHO.	H_2SO_4.
1.	Ni	Ni	Mg	Al	Al	Mg	Ag	Al
2.	Cu*	Fe†	Al*	Fe	Mg	Ni	Cu	Ni†
3.	Fe	Cu	Ni	Ni	Ni	Al	Cd	Pb
4.	Pb*	Sn	Ag	Mg	Fe	Fe†	Fe†	Mg
5.	Al*	Zn	Fe	Sn	Pb	Sn	Pt	Fe*
6.	Mg	Cd	Sn	Ag	Cd	Cd	Au	Sn
7.	Sn	Al	Pb	Cu	Sn	Cu	Pd	Cd
8.	Cd*	Mg	Cu	Au	Pd	Pb	Pb	Cu
9.	Zn	Au	Au	Pb	Ag	Zn	Zn	Pd
10.	Pt	Ag	Pd	Pd	Au	Au*	Sn†	Zn
11.	Au	Pd	Cd	Cd	Cu	Ag*	Mg	Au
12.	Ag	Pt	Zn	Zn	Zn	Pt	Ni*	Ag
13.	Pd	Pb	Pt	Pt	Pt	Pd	Al*†	Pt

Table IV—(continued).

	Na_2SO_4.	HNO_3.	$HClO_3$.	$MgSO_4$.	K alum.	Am alum.
1.......	Ni	Al	Al	Al*	Al*	Al
2.......	Fe	Mg	Mg	Ni	Ni	Ni
3.......	Au	Fe	Cd*	Fe	Mg	Fe
4.......	Mg	Ni†	Ni	Pt	Fe	Sn
5.......	Sn	Cu	Fe	Sn	Sn	Mg†
6.......	Cd	Au	Pb	Cd	Pd	Cd
7.......	Cu*	Ag	Zn†	Pd	Pb	Ag
8.......	Zn	Pb	Pd	Au	Pt	Pt
9.......	Pb	Cd	Sn	Pb	Ag	Pd
10.......	Al	Pd	Cu*	Cu	Cd	Zn
11.......	Ag	Sn	Ag†	Ag†	Cu*	Cu
12	Pd	Zn	Au	Mg	Zn	Au
13.......	Pt	Pt	Pt	Zn	Au	Pb*

* Indicates a gradual reversal with *one* metal only.

† Indicates that a temporary reversal occurred when the currents from the two contiguous metals (*i.e.*, the one marked and the one *above* it) were opposed. A few reversals also took place with non-contiguous metals.

Remarks.—This table contains 23·42 per cent., or a proportion of 1 to 3·27, of instances of metals thermo-electro-negative in liquids. A comparison of it with Table I shows that a large increase of strength of liquid increased the number of instances of metals thermo-positive in liquids from 199 to 219, or from 69·58 to 76·58 per cent., thereby further showing the influence of the liquid upon the apparent thermo-electric character of the metal, and indicating that increased concentration of the solution was usually accompanied, either by an increase of thermo-electro-negative, or decrease of thermo-electro-positive property of the liquid; or that the metals, under the new condition, were more electro-positive. No thermo-electric reversals occurred during the experiments for making Tables I and IV, because the metals were not *gradually* heated.

Increased strength of liquid also greatly altered the order of the series in every case, and produced a very large number of reversals of position or of relative electro-motive force of the metals; out of the total 286 instances, 36 only were not reversed. With some particular strength of liquid, therefore, intermediate between those employed in forming the two tables, the currents from each two metals, which were reversed in position, must balance each other. Many reversals of

thermo-electric sign also of single metals occurred, and in all these, viz., 64, with some intermediate strength of liquid, and the two pieces of metal at 60° and 160° F. respectively, no thermo-electric current would be produced. Magnesium suffered a very great depression of electro-motive force from near the top to quite the bottom of the series in solution of potassic fluoride by increased strength of the liquid; and many other great changes of position of metals in other solutions were similarly produced.

The depression of magnesium to about the bottom of the thermo-electric series in nine different liquids in Table I, and about the same number in Table IV, may have been due to the already-mentioned (p. 255) circumstance that in all the solutions, except those of acids and salts of acid reaction, magnesium becomes coated with an adherent film of white basic salt. In not one of the solutions of acids or acid salts was magnesium thus coated, or its position depressed. On the other hand, the coating of basic salt had no such effect in the solution of weak potassic fluoride; if also the coating caused the magnesium to be thermo-electro-negative in so many liquids, the hot magnesium would probably have been positive at the first instant of immersion, before the coating had time to sufficiently form, but it was not. (See Table I, Notes.)

Both in weak and strong solutions, silver, copper, palladium, and cadmium usually gave thermo-currents of the least electro-motive force, and aluminium and nickel were conspicuously thermo-positive and platinum and magnesium frequently the most thermo-negative.

Many of these results agree with the circumstance that with unequally heated metals in liquids, great changes of strength, and even reversals of current, frequently occur without any apparent cause, but probably in consequence of alteration of chemical composition and

Table V.

Liquid.				Metals.	Volts.
20 minims of H_2SO_4	to each	ounce of water,	unboiled........	Pt −	·0792
,, ,,	,,	,,	boiled..........	,, −	·0579
10 grains of K_2SO_4	,,	,,	unboiled	Ni +	·0996
,, ,,	,,	,,	boiled	,, +	·1249
,, ,,	,,	,,	,, repeated..	,, +	·1000
6¼ minims of HCl	,,	,,	unboiled	Cu +	·1034
,, ,,	,,	,,	boiled	,, +	·1210

strength of the liquid by irregularities of diffusion at the surfaces of the metals; this is largely proved by the great effect which is usually produced by stirring the liquid. Bouty refers such variations partly to the presence of dissolved air and partly to movements of flow of

the liquid. (See "Comptes Rendus, Académie des Sciences, Paris," vol. xc, 1880, pp. 917—920.)

Some experiments were made to ascertain the effect of pre-boiling the liquid upon the degree of thermo-electric potential. With the two pieces of metal at 60° and 160° F. respectively, the results are as given in Table V (p. 261).

With the object of ascertaining the influence of degree of concentration of the liquid upon the thermo-electric potential, the degrees of potential of nearly all the currents from the top and bottom members of the series in Table IV were measured by the method of balance (see p. 257); those only being omitted which were very

Table VI.—Potentials of Thermo-couples in Strong Solutions.

Metal.	Liquid.	Strong solution. Volts.	Weak solution. Volts.
Al	$MgSO_4$	·6034	
"	K alum	·5700	
"	Am alum	·5084	
Sn	Na_2HPO_4	·4469	
Al	HNO_3	·3253	·4317 = ·1064 decrease.
Sn	KI	·2846	
Ag	KF	·2084	
Ni	KCl	·1861	·1439 = ·0422 increase.
Al	$HClO_3$	·1652	·3051 = ·1399 decrease.
"	H_2SO_4	·1634	·2778 = ·1144 "
"	KNO_3	·1615	
"	KBr	·1438	
Ni	Na_2SO_4	·1361	·0975 = ·0386 increase.
Ag	KHO	·1349	
Sn	KCy	·0971	·1827 – ·0856 decrease.
Mg	Tartaric acid	·0822	
Ni	HCl	·0790	·0978 = ·0188 "
Mg	K_2SO_4	·0712	
"	Formic acid.	·0576	
Ni	NaCl	·0299	
Fe	Oxalic acid	·0294	Total 4·4844. Average ·2135.
Cd	KI	·0154	
Pb	Am alum	·0182	
Pd	Tartaric acid	·0384	
Au	K alum	·0448	
Pt	H_2SO_4	·0503	·1403 = ·0900 decrease.
Pd	HCl	·0707	
Pt	K_2SO_4	·0906	
Mg	KF	·0944	
Al	KHO	·1105	
Pt	Oxalic acid	·1404	
Mg	Na_2HPO_4	·1425	
Pt	HNO_3	·1673	
Mg	K_2CO_3	·2478	·1498 = ·0980 increase.
Pt	$HClO_3$	·3644	
			Total 1·5955. Average ·1139.

variable. The pieces of metal were in each case at 60° and 160° F. respectively. The foregoing Table VI contains the results :—The degrees of potential of those cases in which the same metals were used with the weaker liquids are inserted for convenience of comparison.

Remarks.—Several of the remarks made on Table III (p. 258), apply to this one. Whilst also there were six cases in which the

Table VII.—Potentials of Thermo-electric Couples.

Liquid.	Metal.	Volts.
6¼ minims of H_2SO_4 to each ounce of water	Pt −	·1773
	Ag −	·0880
	Cu +	·0525
31¼ " " " "	Pt −	·3347
	Ag −	·0484
	Cu +	·0108
1 volume of H_2SO_4 and 9 volumes of water	Pt −	·1089
	Ag −	·0415
	Cu continually reversing	
1 " " 4 " "	Pt −	·2234
	Ag −	·0488
	Cu −	·0138
·72 minim of strong selenic acid to each ounce of water..	Pt −	·1541
1·45 " " " " " ..	" −	·1273
2·9 " " " " " ..	" −	·0747
5·75 " " " " " ..	" −	·0293
11·5 " " " " " ..	" +	·0330
2·5 grains of chromic acid " " ..	Pt −	·0433
5·0 " " "	" −	·1416
10 " " "	" −	·0137
12½ grains of glacial phosphoric acid to each ounce of water	Pt −	·1815
25 " " " " "	" −	·2027
50 " " " " "	" −	·1687
1¼ minim of HNO_3 to each ounce of water..............	Pt −	·2636
6¼ " " " "	" −	·2617
31¼ " " " "	" +	·2850
156¼ " " " "	" +	and − variable
312½ " " " "	" −	·2354
2·5 grains of iodic acid " "	Pt −	·0484
5 " " " "	" −	·0633
20 " " " "	" −	·0451
60 " " and 62½ minims of H_2SO_4 to each ounce of water	Pt −	·2895
120 grains of iodic acid and 125 minims of H_2SO_4 to each ounce of water	" −	·2120

thermo-electric potential was decreased by increased strength of liquid, there were only three in which it was increased,* and both the total and average amount of decrease was greater than that of increase.

The determinations in Table VII of the thermo-electric potentials of metals in liquids, the pieces of metal being at 60° and 160° F. respectively, illustrate more fully the influence of strength of liquid.

Remarks.—In fifteen cases the thermo-electric potential was decreased, and in eight increased by additional strength of liquid, and the total amount of decrease was greater than that of increase. As greater strength of solution was usually attended by decreased thermo-electric potential, it would be interesting to ascertain whether this was related in any degree to the diminished specific heat of the liquid, or to changes of diffusive power of its constituents.

The following additional determinations of thermo-electric potential by the method of balance were also made with electrodes at 60° and 160° F. respectively.

Table VIII.—Thermo-electric Potentials.

Liquid.	Metal.	Volts.
A half saturated solution of pure cupric sulphate.....	Pt −	·0177
Ditto ditto ditto	Ag −	·0547
Ditto ditto ditto	Cu −	·0617*
10 grains of ammonic nitrate to each ounce of water	Pt +	·0882
10 " " phosphate " "	" +	·0057
10 " sodic diphosphate " "	Sn +	·2437
13¾ " " selenate " "	Pt −	·0688
10 " " hyposulphite " "	" +	·0545
10 " chloride of cobalt " "	" −	·0288
100 " potassic fluoride " "	Ag +	·1045
100 " " hydrate " "	" +	·1464
100 " " cyanide " "	Pt +	·0970
10 " " ferrocyanide " "	" +	·0527

* This = ·00034 volt for 1° C. Bouty obtained ·0007 of one Daniell's element. ("Compt. Rend.," vol. xc, p. 917.)

No difference of potential was observable on opposing the current from two thermo-electric couples of tin in solution of potassic cyanide, one being composed of large electrodes and the other of small ones. By opposing those from the same metal and liquid, the one couple having a large hot electrode and a small cold one, and the other the reverse, no definite result occurred.

Very few researches have been made on the thermo-electric properties of liquids. H. Wild ("Poggendorff's Annalen," vol. ciii, pp. 353—411, 1858) has shown that when two vertical columns of

* This is a usual circumstance.

different electrolytes at 60° F., of different specific gravities, and not mutually decomposable, are placed end to end upon each other as distinct strata in mutual contact, the lighter one being the uppermost, and the junction of the two is heated, a thermo-electric current is produced. He has also shown that this current varies in strength with the kind of solution and the degree of its concentration.—That the electromotive force of the resulting current is nearly proportional to the difference of temperature up to that of 50° of the portions of liquid at the contact surface.—That with two portions of solution of different degrees of strength of the same salt, the current passes from the weaker to the stronger one.—And that when equal volumes of the two solutions contain chemically equivalent weights of salts of two different metals, the order of the liquids in electric tension series is the same as that of their metals. Further, that the order of the series with solutions of all neutral salts of the type $R.SO_4$, agrees with that of the ordinary chemico-electric series of their metallic bases in the case of salts of K, Na, Mg, Mn, Fe, Ni, CO, Zn, Cu, and Ag, thus:—

Table IX.

Thermo-electric Series.		Chemico-electric Series.
K_2SO_4	......	K
$MgSO_4$	......	Mg
$ZnSO_4$	......	Zn
$FeSO_4$	......	Fe
$NiSO_4$	......	Ni
$CuSO_4$	......	Cu

Solutions of the salts KCl, KBr, KI, also follow a similar order, but those agreeing with the formula $R_2O_3SO_3$ do not obey this rule, nor do acids in general.

E. Becquerel also ("Annales de Chimie et de Physique," 4th series, 1866, vol. vii, pp. 392—397) similarly heated the mutual contact portions of two electrolytes about 10° to 80°, and found the following effects: A saturated solution of sulphate of copper was positive to a mixture of 1 part of sulphuric acid and 19 parts of water; water acidulated with hydrochloric acid was positive to a solution of sodic chloride, rendered alkaline by caustic potash; strong nitric acid was positive to a solution of 1 part of caustic potash in 10 parts of water; solution of cadmium sulphate was positive to a dilute one of ammonic chloride, and no current was produced by heating the junction of a solution of persulphide of potassium and a diluted one of the same salt.

One of the chief objects of this research being to obtain new knowledge respecting the relation of thermo-electric action of liquids and metals to chemico-electric action, the chemico-electric

positions in series of all the foregoing combinations of metals and liquids were determined at the temperatures of 60° and 160° F. respectively, in order to compare the two series, and ascertain how far the differences in chemico-electric position at different temperatures agreed with the thermo-electric relations of the particular combinations of metal and liquid at those temperatures. All the solutions were of the same degree of strength as those used in forming the thermo-electric series of Table I. The experimental results are shown in the following table:—

Table X.—Chemico-electric Series at 60° and 160° F.

	1 KCy.		2 KF.		3 KBr.		4 KNO_3.		5 K_2CO_3.	
	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.
1	Mg	Al	Al	Al	Mg	Mg	Mg	Mg	Mg	Al
2	Al	Zn	Mg	Mg	Zn	Zn	Zn	Zn	Al	Mg
3	Cu	Cu	Zn	Zn	Cd	Cd	Cd	Cd	Sn	Sn
4	Zn	Cd	Cd	Sn	Al	Al	Pb	Fe	Pb	Zn
5	Cd	Sn	Sn	Cd	Pb*	Pb	Fe	Pb	Zn	Pb
6	Sn	Mg	Fe	Fe	Fe	Fe	Sn	Al	Cd	Cd
7	Ag	Ag	Pb	Pb	Sn*	Sn	Al	Sn	Fe	Fe
8	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Cu
9	Pb	Pd	Cu	Cu	Cu	Cu*	Ag	Cu	Cu	Ni
10	Au	Au	Pd	Pd	Ag	Ag	Cu	Pd	Pd	Pd
11	Pd	Pb	Pt	Pt	Pd	Pd	Pd	Ag	Au	Au
12	Fe	Fe	Ag	Ag	Au	Au	Au	Au	Pt	Pt
13	Pt	Pt	Au	Au	Pt	Pt	Pt	Pt	Ag	Ag

	6 Na_2PO_4.		7 KI.		8 NaCl.		9 HCl.		10 KCl.	
	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.
1	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg
2	Zn	Al	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn
3	Pb	Cd	Cd	Cd	Al	Al	Cd	Cd	Al	Al
4	Cd	Zn	Pb	Fe	Cd	Cd	Al	Al	Cd	Cd
5	Sn	Sn	Al	Pb	Pb	Fe	Fe	Fe	Pb	Pb
6	Al	Pb	Sn	Al	Fe	Pb*	Sn	Sn	Fe	Fe
7	Fe	Fe	Fe	Sn	Sn	Sn	Pb	Pb	Sn*	Sn
8	Cu	Cu	Cu	Cu	Ni	Cu	Ni	Ni	Cu	Cu
9	Ni	Ni	Ag	Ag	Cu†	Ni	Cu	Cu	Ni	Ni
10	Pd	Pd	Ni	Ni	Ag	Ag	Ag	Ag	Ag	Ag
11	Pt	Pt	Au	Au	Au	Au	Au	Au	Au	Pd
12	Au	Au	Pd	Pd	Pd	Pd	Pt	Pt	Pd	Au
13	Ag	Ag	Pt	Pt	Pt	Pt	Pd	Pd	Pt	Pt

Table X—(continued).

	11 K_2SO_4.		12 Oxalic acid.		13 Formic acid.		14 Dextro-tartaric acid.		15 KHO.	
	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.
1	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Al	Al
2	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn*	Zn
3	Cd	Cd	Cd	Al	Cd	Cd	Cd	Cd	Sn	Mg
4	Fe	Fe	Sn	Cd	Fe	Fe	Fe	Al	Mg	Sn
5	Pb	Al*	Al	Sn	Pb	Al	Sn*	Fe	Pb	Cd
6	Sn	Pb	Fe	Fe	Sn	Sn	Pb	Sn	Fe	Pb
7	Al	Sn	Pb	Pb	Al	Pb	Al	Pb	Cd	Fe
8	Cu	Ni	Ni	Ni	Ni	Ni	Ni	Ni	Cu	Cu
9	Ni	Cu	Cu	Cu	Cu	Cu	Cu	Cu	Ni	Ni
10	Au	Ag	Ag	Ag	Au	Ag	Ag	Ag	Au	Au
11	Pd	Pd	Pd	Au	Ag	Au	Au	Au	Pd	Pt
12	Ag	Au	Au	Pd	Pt	Pd	Pd	Pd	Pt	Pd
13	Pt	Pt	Pt	Pt	Pd	Pt	Pt	Pt	Ag	Ag

	16 H_2SO_4.		17 Na_2SO_4.		18 HNO_3.		19 $HClO_3$.		20 $MgSO_4$.	
	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.	Cold.	Hot.
1	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg	Mg
2	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn	Zn
3	Cd	Al	Cd	Cd	Cd	Al	Cd	Al	Al	Al
4	Fe	Cd	Pb	Fe	Fe	Cd	Fe	Cd	Cd	Cd
5	Sn*	Fe	Fe	Pb*	Pb	Fe*	Al	Pb	Pb	Fe
6	Pb	Sn	Al*	Al	Al	Pb*	Sn	Sn	Fe	Pb
7	Al	Pb	Sn	Sn	Sn	Ni	Pb	Fe	Sn	Sn
8	Ni	Ni	Ni	Ni	Ni	Sn	Ni	Ni	Ni	Ni
9	Cu	Cu	Cu	Ag	Cu	Cu	Cu	Cu	Cu	Cu
10	Au	Ag	Ag	Cu	Ag	Au	Ag	Ag	Au	Ag
11	Pd	Pt	Pd	Pd	Pt	Pt	Au	Au	Ag	Au
12	Ag	Au	Pt	Pt	Au	Pd	Pd	Pd	Pd	Pd
13	Pt	Pd	Au	Au	Pd	Ag	Pt	Pt	Pt	Pt

Table X—(continued).

	21 Potash alum.		22 Ammonia alum.*	
	Cold.	Hot.	Cold.	Hot.
1	Mg	Mg	Mg	Mg
2	Zn	Zn	Zn	Zn
3	Cd	Al	Cd	Cd
4	Fe	Cd	Fe	Fe
5	Pb	Fe	Al	Al
6	Al	Sn*	Pb	Sn
7	Sn	Pb	Sn	Pb
8	Ni	Ni	Ni	Ni
9	Cu	Cu	Cu	Cu
10	Pt	Ag	Pd	Pd
11	Au	Pt	Ag	Ag
12	Ag	Au	Au	Au
13	Pd	Pd	Pt	Pt

* Indicates a temporary reversal with the two contiguous metals, *i.e.*, the one marked and the one *above* it.

† Indicates that a current occurred on contact only between the marked one and the next above it.

Remarks.—The currents obtained from two contiguous base metals were usually much stronger than those from contiguous noble ones. The influence of rise of temperature in altering the relative positions of the various metals in the series may be rendered more evident by drawing cross lines between those which are altered. It might have been supposed that as chemical action is usually increased by heat, the hot piece of metal would in nearly every instance have been either more electro-positive or less electro-negative than the cold one, and that the orders of the chemico-electric series of metals with each liquid at 160° would have been nearly always the same as at 60° F. The results exhibited by Table X, however, show that out of 22 liquids, in two only, viz., dilute hydrochloric acid and solution of potassic bromide, was the order of the hot series of metals exactly the same as that of the cold one. In all these cases of voltaic as well as of thermo-electric action, change of temperature was attended by change of electric state.

By tabulating separately, and comparing the numbers of instances in which the respective metals occur on the several lines of Tables I and X, it will be at at once seen that in the thermo-electric series the metals generally, whilst occupying the same parts of the table as those

of the chemico-electric ones, are much more distributed, those of each particular metal occupying about double the number of lines. This probably indicates that in these thermo-electric actions voltaic influence modified the effects; also that heating one of the pieces of metal only of a voltaic pair usually increased the amount of their electric difference, making most metals more positive and some more negative, whilst heating the second one also, usually neutralised to some extent the effect of heating the first one only.

Tabulating the members thus also shows that gold and platinum are the least distributed of the metals of the thermo-electric series, they are also the least corrodible. Platinum, gold, palladium, and silver occupy in all cases the lower half only of the table in the voltaic series, and in nearly all cases also in the thermo series. Aluminium, tin, and iron preponderate in the upper section of both tables. These facts also indicate that voltaic influence operates in most of these thermo-electric actions. An examination of Table II further supports this conclusion.

As the electrical effect of heating a voltaic couple may be viewed as being in a greater degree composed of the united consequences of heating each of the two metals separately (see pp. 270, 271, and 281), I have compared the chemico-electric series in the weak solutions at 60° F., Table X, p. 267, side by side with the thermo-electric ones of the same metals in the same liquids of Table I, in a similar manner as the two chemico series are themselves compared. By such a comparison it was found that magnesium, zinc, and cadmium suffered usually very great depressions of position and electromotive force in the series by the influence of heat (see pp. 276, 278). In such a table of comparison, magnesium was depressed 21 times through a total of 144 places; zinc was lowered 20 times through a sum of 123, and raised through 1 only; and cadmium sank 17 times through 87, and rose 3 times through a total of 8. All these numbers follow the same order as the specific gravities, atomic weights, and specific heats of those metals. All the other metals behaved in a very different manner from this. The same effects may be rendered evident in another way by tabulating the number of instances in which each of those metals respectively occurs in the several lines of Table I and of the cold columns of Table X. Tables I and II also show that, notwithstanding the highly volta-positive character of this group of metals, magnesium was thermo-negative 10, cadmium 8, and zinc 7 times, and that they were nearly as often thermo-negative as silver. This effect is least conspicuous with the weak solutions of potassic chloride, carbonate, and hydrate, and was not manifestly affected by increased strength of liquid. The similar behaviour in this respect of the whole of the magnesium group of metals, suggests the conclusion that the frequent and great depressions in position of magnesium in the thermo-electric

series (Tables I and IV, pp. 253 and 259) were not always due to a film of insoluble matter (pp. 261, 270, and 278). I have not examined whether this phenomenon is related to the great thermal capacity and expansibility by heat of those metals. The liquid in which the metals generally were least altered in position by heating one of them was the weak solution of potassic chloride, it suffered also very few voltaic reversals by rise of temperature (see Table X, p. 266).

By making similar comparisons of the thermo series of Table I with the " hot " chemico series of Table X, it was observed, that whilst heating only one metal of a voltaic pair to 160° F. depressed the relative positions of the magnesium group of metals a total of 354 places, and raised them only 9 (see p. 269); heating subsequently the second one in addition, raised them from the latter position a total of 345 places, and depressed them only 5, and therefore restored them nearly 98 per cent. towards their former positions. This appears to be in harmony with the fact that the orders of the voltaic series at 60° and 160° F. are not widely different (see Table X, p. 260. By similar tabular comparisons and reckonings, the electrical effect of heating the first electrode was found to be usually a little greater than that of heating the second one.

The extremely negative position of magnesium in many thermo-electric series, both with weak and strong solutions, does not appear to be explicable either by its strongly volta-positive character, its own thermo-positive character amongst metals, or by the thermo-electric properties of the liquids, but requires the assumption of some other cause operating. The thermo-electric positions of several other metals, such as aluminium, nickel, and palladium, &c., in particular liquids, are also, though in a less degree, inexplicable by these causes alone.

Many of the metals in Table X (p. 260) were reversed in their order of position in the series by rise of temperature; the total number not reversed was 174, and of reversed 112, or 1·55 to 1·0 = 64·51 per cent. The reversals varied in number in different liquids; most occurred with nitric and sulphuric acids and potassic cyanide (8 in each). Every metal suffered reversals; the number was greatest with aluminium (30) and lead (23), and least with zinc and nickel (4). The metals which most frequently crossed each other were aluminium with lead and with iron, each seven times. Reversals were less frequently caused in chemico-electric series by a rise of temperature of 100° F. than in a thermo-electric series by a difference of strength of liquid, the proportion in the former case being 174 out of 286, and in the latter 250 out of 286 (see p. 258).

Regarding the effect of heating two different metals simultaneously in the chemico-electric experiments (shown in Table X, p. 260), as being chiefly composed of the two effects of heating the same metals separately (as shown in the thermo-electric series of Table I), I have

compared the relative positions in Table X of the metals which have crossed each other by simultaneous heating to 160° F. with those of the same metals in Table I, as determined by heating them separately to that temperature in the same liquids. I thus found that out of the total 83 instances of crossing 72 are explicable in this manner, and the remaining 9 are but feeble exceptions, both as regards their relative positions in the chemico- and in the thermo-electric series, and also as regards their relative degrees of electromotive force, 7 out of the 9 couples being composed of noble metals, which give relatively feeble currents. No instance occurred in which the reversal was produced by alteration of potential of one metal only; 32 happened in which it was due to one metal increasing and the other decreasing in that power; 37 in which it was due to both metals increasing simultaneously, but at different rates; and 5 in which it was a result of simultaneous but unequal diminution of potential in the two metals. The differences of order of the chemico-electric series at 60° and 160° F. in Table X, and the coincident reversals, are therefore results of the combined effect of heat upon the two individual metals, as shown in Table I. In the cases of reversal, the number of instances in which a metal becomes positive by rise of temperature was more than twice as great as those in which it became negative; this agrees with the proportion of thermo-positive and negative members in Table I (see pp. 253 and 254), and with the statement that heat usually increases electro-positive activity.

It is evident that as in certain cases a given metal is chemico-electro-positive at 60° to another metal, whilst at 160° F. it is negative, it must at some intermediate temperature be neutral to it and produce no current; the electric potentials of the two metals must at that point be equal and opposite. A number of experiments were therefore made in order to ascertain the temperatures of these neutral points at which reversal took place. The solutions employed were all of them of the same composition as those used in determining the chemico-electric series (p. 269). The following are the results:—In *Potassic cyanide:* Mg and Al, 80° F.; Mg and Zn, 146°; Pb and Pd, 84°. *Potassic fluoride:* Sn and Cd, 146°. *Potassic nitrate:* Pb and Fe, 65°; Al and Sn, 90°; Ag and Pd, 182°. *Potassic carbonate:* Mg and Al, 106°. *Sodic diphosphate:* Al and Cd, 150°; Al and Zn, 124°; Al and Pb, 100°; Al and Sn, 120°. *Potassic iodide:* Fe and Pb, 120°; Fe and Al, 160°. *Sodic chloride:* Fe and Pb, 60°; Cu and Ni, 72°. *Potassic chloride:* Au and Pd, 65°. *Potassic sulphate:* Pb and Al, 170°; Ni and Cu, 64°; Ag and Pd, 100°. *Oxalic acid:* Al and Cd, 152°; Al and Sn, 122°; Au and Pd, 62°. *Formic acid:* Al and Sn, 176°; Al and Pb, 185°; Pd and Pt, 110°. *Dextro-tartaric acid:* Al and Fe, 177°; Al and Sn, 182°; Al and Pb, 154°. *Potassic hydrate:* Cd and Pb, 138°; Cd and Fe, 68°. *Sulphuric acid:* Al and

Cd, 154°; Al and Fe, 110°; Al and Sn, 100°; Al and Pb, 68°; Pt and Au, 185°. *Sodic sulphate:* Fe and Pb, 62°; Ag and Cu, 176°. *Nitric acid:* Al and Cd, 140°; Al and Fe, 116°; Al and Pb, 86°; Au and Pt, 130°; Ag and Pt, 64°. *Chloric acid:* Al and Cd, 144°; Al and Fe, 120°; Pb and Fe, 184°. *Magnesic sulphate:* Fe and Pb, 65°; Ag and Au, 182°. *Potassic alum:* Al and Cd, 140°; Al and Fe, 132°; Al and Pb, 130°; Sn and Pb, 182°; Ag and Pt, 70°. *Ammonic alum:* Sn and Pb, 102°. The cases of chemico-electric reversal in which both metals diminished in potential were: Au and Pd in potassic sulphate, also in oxalic acid; Fe and Sn in chloric acid; Au and Ag in magnesic sulphate; and Pb and Al in potassic alum. As visible chemical corrosion of metal is not necessary to thermo-electric action of metals in liquids, reversals occurred with non-corroded, as well as with corroded metals.

Very slight circumstances altered the position of a chemico-electric reversal point in the scale of temperature, and even caused it to pass beyond the limits of the scale and of observation. A different sample of metal or of salt, and other circumstances, often produced this effect; the results are therefore very variable.

Out of the total 83 cases of reversal obtained by sudden immersion of the pairs of metals, first at 60° and then at 160° F. (see Table X), only 54 were reobtained on heating the metals gradually in the liquids, and 11 of those so obtained occurred at temperatures higher than those employed when the metals were suddenly heated. Gradually heating the metals appeared therefore to cause in some cases the reversals either to disappear or to occur at a higher temperature (probably in consequence partly of polarisation). This effect was more common in a solution of potassic cyanide than in dilute sulphuric or nitric acids.

It may be observed also with regard to some of these cases that the temperatures at which the reversals took place were usually lower the weaker the electromotive force; this was the case with aluminium in relation to cadmium, iron, tin, and lead in dilute sulphuric acid; also with that metal in relation to cadmium, iron, and lead in nitric acid; and less distinctly in solution of potassic alum and other liquids.

The influence of heat upon the chemico-electric series of Table X was further examined by measuring the degrees of electric potential, both at 60° and 160° F., of the extreme top and bottom metals in the respective liquids of that table. The results are given in Table XI.

Remarks.—In 12 cases the voltaic potential was increased, and in 6 decreased; the total amount of increase was 5 times that of decrease, and the average proportion of increase for the 18 instances was ·10 volt for 100° F. rise of temperature. By comparing these cases with the thermo-electric positions of the same metals caused by the same difference of temperature in Table I, the two were found

Table XI.—Influence of Temperature on Potential of Voltaic Couples.

Solutions.	At 60° F.	At 160° F.	Increase.	Decrease.
	Volts.	Volts.		
1. KCy	Mg ± Pt .. 1·48	Al ± Pt .. 1·54	..	..
2. KF.......	Al „ Au.. 1·11	„ „ Au .. 1·80	·69	..
3. KBr	Mg „ Pt .. 1·67	Mg „ Pt .. 1·75	·08	..
4. KNO_3......	„ „ „ .. 1·35	„ „ „ .. 1·59	·24	..
5. K_2CO_3	„ „ Ag .. 1·67	Al „ Ag .. 1·70	..	..
6. Na_2HPO_4...	„ „ „ .. 1·51	Mg „ „ .. 1·53	·02	..
7. KI	„ „ Pt .. 1·58	„ „ Pt .. 1·44	..	·14
8. NaCl	„ „ „ .. 1·50	„ „ „ .. 1·53	·03	..
9. HCl	„ „ Pd .. 1·61	„ „ Pd .. 1·70	·09	..
10. KCl	„ „ Pt .. 1·44	„ „ Pt .. 1·53	·09	..
11. K_2SO_4	„ „ „ .. 1·50	„ „ „ .. 1·48	..	·02
12. Oxalic acid .	„ „ „ .. 1·73	„ „ „ .. 1·88	·15	..
13. Formic „ .	„ „ „ .. 1·84	„ „ „ .. 2·08	·24	..
14. Tartaric „ .	„ „ „ .. 1·68	„ „ „ .. 1·84	·16	..
15. KHO	Al „ Ag .. 1·37	Al „ Ag .. 1·28	..	·07
16. H_2SO_4	Mg „ Pt .. 1·92	Mg „ Pd .. 1·95	..	..
17. Na_2SO_4.....	„ „ Au .. 1·64	„ „ Au .. 1·61	..	·03
18. HNO_3......	„ „ Pd .. 1·81	„ „ Ag .. 1·88	..	..
19. $HClO_3$	„ „ Pt .. 1·73	„ „ Pt .. 2·13	·39	..
20. $MgSO_4$.....	„ „ „ .. 1·75	„ „ „ .. 1·64	..	·11
21. K alum	„ „ Pd .. 1·81	„ „ Pd .. 1·73	..	·08
22. Am alum...	„ „ Pt .. 1·70	„ „ Pt .. 1·84	·14	..

to harmonise in 13 of the cases, but not in the remaining 5. These results also agree with the fact that heat usually increases electric potential of metals in liquids. The influence of increase of temperature on chemico-electromotive force was usually the reverse of that of increased strength of liquid upon thermo-electric potential (see pp. 264, 268).

The following additional determinations of the chemico-electric potential of two different metals, and with different liquids, all at about 60° F., by the balance method, were made:—

Table XII.—Chemico-electric Potentials.

Liquid.	Metals.	Volts.
25 grains of KF in one ounce of water	Pt + Ag −	·0157
50 „ „ „ „	„ + „ −	·0447
100 „ KHO „ „	„ + „ −	·0354
50 „ K_2CO_3 „ „	„ + „ −	·0195
50 „ Na_2HPO_4 „ „	„ + „ −	·0024
40 „ K_2SO_4 „ „	Ag + Pt −	·0266
10 „ KCl „ „	Sn + Cu −	·2400

As rise of temperature diminishes in certain cases the chemico-electric potential of a metal in an electrolyte (see Table XI, p. 273), this fact may suggest the idea that when a metal is thermo-electro-negative in a particular liquid its degree of rapidity of corrosion is lessened by rise of temperature. This question was subsequently investigated (see p. 285).

By first comparing the orders of the chemico-electric series (Table X) of metals in hot solutions of potassic bromide and chloride, side by side with each other, and then those in hot solution of potassic cyanide and dilute sulphuric acid, similarly, the influences of small and of great difference of chemical composition of the liquid were conspicuously seen. In the former case only four of the metals were altered in position, whilst in the latter ten were displaced. The differences also between the orders of the series obtained with dilute hydrochloric acid and solution of potassic chloride, each at 60° F., were much greater than between those obtained with that acid or that salt at 60° and 160° F. A much greater effect was produced in these cases by a difference of liquid than by one of 100° F. of temperature.

With the object of ascertaining the effect of increased strength of liquid upon the order of the chemico-electric series of metals in electrolytes at 60° F., a similar set of experiments to those employed in constructing Table X were made. The strengths of solutions were the same as those used in making the thermo-electric determinations of Table IV (p. 259).

Table XIII.—Chemico-electric Series of Metals in Strong Solutions (at 60° F.).

	KCy.	KF.	KBr.	KNO_3.	K_2CO_3.	Na_2HPO_4.	KI.	NaCl.
1.......	Zn	Al	Mg	Mg	Mg	Mg	Mg	Mg
2.......	Mg	Zn*	Zn	Zn	Al	Zn	Zn	Zn
3.......	Al	Mg	Cd	Cd	Pb*	Cd	Cd	Al
4.......	Cu	Cd	Al	Pb	Zn	Al	Pb	Cd
5.......	Cd	Sn	Pb	Fe	Cd	Pb	Al*	Pb
6.......	Sn	Pb	Sn	Al*	Sn	Fe	Fe	Sn
7.......	Ag	Fe	Fe	Sn	Fe	Sn	Ag*	Fe
8.......	Ni	Cu	Cu	Ni	Cu	Cu	Cu	Cu
9.......	Au	Pd	Ag	Ag	Ni	Ni	Sn	Ag
10.......	Pb	Au	Ni	Cu	Pd	Pd	Pd	Ni
11.......	Fe	Pt	Pd	Pd	Au	Pt	Au	Au
12.......	Pd*	Ni	Au	Au	Pt	Au	Ni	Pd
13.......	Pt	Ag	Pt	Pt	Ag	Ag	Pt	Pt

Table XIII.—(continued).

	HCl.	KCl.	K_2SO_4.	Oxalic acid.	Formic acid.	Dextro-tartaric acid.	KHO.	H_2SO_4.
1.......	Mg	Mg	Mg	Mg	Mg	Mg	Al	Mg
2.......	Zn	Zn	Zn	Zn	Zn	Zn	Zn*	Zn
3.......	Al	Cd	Cd	Cd	Cd	Cd	Sn*	Cd
4.......	Cd*	Al	Al	Sn	Fe	Fe	Mg	Fe
5.......	Sn	Fe	Fe	Al	Sn	Sn	Cd	Sn
6.......	Fe	Pb	Pb	Fe	Pb	Pb	Fe	Pb
7.......	Pb	Sn	Sn	Pb	Al	Al	Pb	Al
8.......	Ni	Cu	Ni	Ni	Ni	Ni	Cu	Ni
9.......	Cu	Ni	Cu	Cu	Cu	Cu	Ni	Cu
10.......	Ag	Ag	Ag	Ag	Au	Ag	Ag	Au
11.......	Au	Au	Pd	Au	Ag	Au	Pt	Pd
12.......	Pd	Pd	Au	Pd	Pd	Pd	Au	Ag
13.......	Pt	Pt	Pt	Pt	Pt	Pt	Pd	Pt

	Na_2SO_4.	HNO_3.	$HClO_3$.	$MgSO_4$.	K alum.	Am alum.
1.......	Mg	Mg	Mg	Mg	Mg	Mg
2.......	Zn	Zn	Zn	Zn	Zn	Zn
3.......	Cd	Cd	Cd	Cd	Cd	Cd
4.......	Fe	Pb	Al	Fe	Fe	Fe
5.......	Pb*	Fe	Pb	Al	Pb	Pb
6.......	Sn	Sn*	Sn	Pb	Sn	Sn
7.......	Al	Al	Fe	Sn	Al	Al
8.......	Ni	Ni	Ni	Ni	Ni	Ni
9.......	Cu	Cu	Cu	Cu	Cu	Cu
10.......	Ag	Ag	Ag	Pd	Ag	Ag
11.......	Pd	Pt	Au	Au	Pt	Pd
12.......	Au	Au	Pd	Ag	Au	Au
13.......	Pt	Pd	Pt*	Pt	Pd	Pt

* Indicates a temporary reversal with the two contiguous metals, *i.e.*, the one marked and the one *above* it.

Remarks.—The usual strength of the current obtained in this series of experiments was but little greater than those obtained with the weaker liquids at the same temperature. Silver was nearly always the most volta-electro-negative metal in alkaline liquids, except in potassic cyanide, whether they were strong or weak (see Tables X and XIII). Copper was remarkably volta-positive, both in hot and cold weak solution of potassic cyanide (see Table X), and in a strong cold one.

Difference of strength of solution, like difference of temperature, altered the order of the series with nearly every liquid. In the same total number of instances (286), the number of reversals of position in the orders, in liquids of four or five times the strength, was 74, whilst that produced by a difference of temperature of 100° F. was 83; therefore the usual amounts of chemico-electric molecular change produced in the metals and liquids by these two causes were not widely different. In twenty-nine cases the reversals produced by increased strength of liquid, occurred with the same metals and liquids as those produced by rise of temperature. Whilst also with the 286 members of the thermo-electric series in Table IV (see p. 260), a variation of strength of liquid greatly altered the sequence, and produced no less than 236 reversals, it caused only a moderate degree of change of order, and only 74 reversals in the chemico-electric series. This conspicuous difference between effects of increased strength of liquid upon the thermo- and chemico-electric series, is perhaps explicable by the circumstance that a thermo-electric couple, consisting of an unequally heated metal and electrolyte, behaves like a feeble voltaic combination of two metals and two liquids, and is a more complex arrangement than a voltaic element of two metals and one liquid, such as was used in forming the above Table XIII.

The influence of strength of solution upon the electric potential of the extreme top and bottom members of the "cold" columns of the chemico-electric series of Table X, was also examined by the method of balance; the liquids employed being the same as those used in ascertaining the influence of the same condition upon the order of the thermo-electric series of Table IV (p. 259). All the solutions were used at 60° F. The results, placed side by side with those obtained with the weaker liquids, are shown in Table XIV.

Remarks.—In twelve cases the electric potential was increased, and in five decreased by increased strength of the solutions. The total amount of increase was 3·55 times that of decrease, and the average proportion of increase for the seventeen instances was ·095 volt per 100° F. rise of temperature. The results when compared with those in Table XI, p. 273, show that the increased strength of the liquid had about the same average degree of effect as a rise of temperature of 100° F. in increasing the chemico-electric potential. The influence of increased strength of liquid upon chemico-electric potential was different from that upon thermo-electric potential, in the former it was usually attended by an increase, and in the latter by a decrease, see Tables VI, VII.

The electric potential of a voltaic couple appears to be in certain cases decreased by rise of temperature (see Table XI, p. 273). This circumstance is a consequence of the fact that by accession of heat many metals become electro-negative, and others electro-positive in

Table XIV.—Influence of Strength of Liquid on Voltaic Potential.

Solutions.	Weak.		Strong.		Increase.	Decrease.
	Metals.	Volts.	Metals.	Volts.		
1. KCy	Mg ±Pt	1·48	Zn ±Pt	1·32	..	..
2. KF	Al ±Au	1·11	Al ±Ag	1·09	..	..
3. KBr	Mg ±Pt	1·67	Mg ±Pt	1·53	..	·14
4. KNO_3	„ ± „	1·35	„ ± „	1·59	·24	..
5. K_2CO_3	„ ±Ag	1·67	„ ±Ag	1·73	·06	..
6. Na_2HPO_4...	„ ± „	1·51	„ ± „	1·46	..	·05
7. KI	„ ±Pt	1·58	„ ±Pt	1·50	..	·08
8. NaCl.......	„ ± „	1·50	„ ± „	1·75	·25	..
9. HCl	„ ±Pd	1·61	„ ± „	1·95	..	..
10. KCl	„ ±Pt	1·44	„ ± „	1·75	·31	..
11. K_2SO_4	„ ± „	1·50	„ ± „	1·68	·18	..
12. Oxalic acid.	„ ± „	1·73	„ ± „	1·61	..	·12
13. Formic „ .	„ ± „	1·84	„ ± „	1·87	·03	..
14. Tartaric „ .	„ ± „	1·68	„ ± „	1·80	·12	..
15. KHO	Al ±Ag	1·37	Al ±Pd	1·48	..	..
16. H_2SO_4	Mg ±Pt	1·92	Mg ±Pt	1·98	·06	..
17. Na_2SO_4.....	„ ±Au	1·64	„ ± „	1·76	..	..
18. HNO_3	„ ±Pd	1·81	„ ±Pd	1·57	..	·24
19. $HClO_3$	„ ±Pt	1·73	„ ±Pt	2·17	·44	..
20. $MgSO_4$.....	„ ± „	1·75	„ ± „	1·82	·07	..
21. K alum	„ ±Pd	1 81	„ ±Pd	1·87	·06	..
22. Am „	„ ±Pt	1·70	„ ±Pt	2·12	·42	..

liquids (see Tables I and IV); when therefore a combination in which the volta-positive metal is thermo-negative, and the negative one is thermo-positive, is heated, the electric potential tends to diminish, notwithstanding that the internal resistance usually decreased. The following are selected cases of this kind, the data of which are derived from comparison of Tables III and VI with Table X, pp. 257, 262, and 266; a large number of additional ones might be obtained by the same method. The amounts of decrease are for a difference of temperature of 100°, ranging from 60° to 160° F.

Table XV.—Influence of Temperature on Voltaic Potential.

Metals.	Liquids.		Volt.	
Pb + and Ni —	in $MgSO_4$	(weak solution)	·1876	decrease.
Mg + „ Cd —	„ KHO	„	·2046	„
Mg + „ Sn —	„ KCy	„	·2518	„
Al + „ Ag —	„ KHO	(strong solution)	·2484	„
Cd + „ Sn —	„ KI	„	·2998	„
Mg + „ Ag —	„ KF	„	·3028	„
Mg + „ Sn —	„ Na_2HPO_4	„	·5894	„

The instances were selected because they were conspicuous ones. The results are probably in some cases partly due to extremely thin films of insoluble matter formed upon the metals (see pp. 255, 261, 270, 278); this assumption of the existence of films will not, however, explain many of the cases in Tables I and IV, when non-corroded metals were rendered negative by being heated. Fifty-six cases of relative diminution of volta-electric potential by rise of temperature are included in the cases of reversal in Table X (see p. 266).

Similar results to the above were arrived at by a more direct method. The chemico-electric potential of each pair of metals was first determined (by the method of balance) at 60° and then at 160° F., by suddenly immersing the pair in the previously prepared liquid. The following were the results:—

Table XVI.—Influence of Temperature on Voltaic Potential.

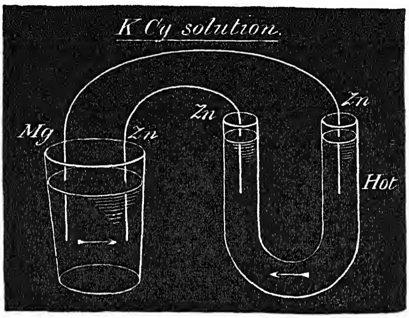
Metals.	Liquids.	Volts at 60° F.	Volt at 160° F.		
Pb + and Ni – in	$MgSO_4$ (weak solution)	·3447	·2364	=	·1083 decrease.
Mg ,, Cd ,,	KHO (,, ,,)	·6189	·4485	=	·1704 ,,
,, ,, Sn ,,	KCy (,, ,,)	1·0070	·0842	=	·9229 ,,
Al ,, Ag ,,	KHO (strong ,,)	1·5083	1·2245	=	·2838 ,,
Cd ,, Sn ,,	KI (,, ,,)	·3313	·2912	=	·0401 ,,
Mg ,, Ag ,,	KF (,, ,,)	·8064	·6032	=	·2032 ,,
,, ,, Sn ,,	Na_2HPO_4 (,, ,,)	1·1087	·2758	=	·8329 ,,
,, ,, Ni ,,	$MgSO_4$ (weak ,,)	1·3469	1·3722	=	·0253 increase.

Remarks.—While each of these determinations except the last one agrees with the former ones in being a case of decrease of electric potential by equal rise of temperature, the amounts of decrease for the corresponding pairs of metals differ very considerably in the two tables, probably in consequence chiefly of the circumstance that in each of the experiments of the former determinations the metals in mutual contact were the same, but at different temperatures, whilst in the latter they were dissimilar ones at the same temperature. When heating a voltaic pair, the heat is applied to two metals, both of which are previously electro-polar by contact with each other, as well as by contact with the liquid; but when heating one junction of a liquid and metal thermo-couple, the metal has not been previously rendered electro-polar by contact with a different one, and is therefore in a somewhat different electric state. Faraday observed similar cases ("Experimental Researches," 1923), viz., that by either heating one or two pieces of silver or of platinum in a cold mixture of 1 part of sulphuric acid and 80 of water, little or no electric current was manifest, but on heating the silver or platinum alone of a voltaic pair of those metals in that liquid, in each case a current occurred. Heating the platinum made it strongly negative, and heating the silver made it weakly positive. These cases illustrate the necessity of considering the effect of voltaic contact, as well as that of chemico-

electric influence, when examining thermo-electric actions of metals in liquids. Additional instances are given on pp. 281, 288.

With the object of ascertaining the electric potential of voltaic currents from pairs of metals in the "cold" columns of liquid of Table X in thermo-electric terms of one of the same metals; the current of two of those metals (contiguous or as near to each other as could be obtained) in one of those liquids at atmospheric temperature, was balanced by a thermo-electric one from two pieces of one of the same metals in a second portion of the same liquid, in a bent glass tube, as shown in the annexed sketch (fig. 2),* and the

FIG. 2.



difference of temperature between the two pieces ascertained at which the balance occurred. Usually, however, the bent tube alone (as in fig. 1, p. 251, but provided with a hot water-bath), containing the two metals, was used. The temporary portion of current was in every case allowed to subside before applying the heat. Cases in which the voltaic current from two contiguous metals was too strong to be balanced by the only applicable thermo-electric one were excluded. The electric potentials at 60° F. of most of the voltaic pairs employed were subsequently determined. The following are the results :—

Table XVII.—Thermo-electric Balance of Chemico-electric Action.

	Chemico-electric						Thermo-electric										
Solution.	Positive.		Negative.		F.		Positive.		F.		Negative.		F.		Difference in F. degrees.		Volt.
KCy	Mg	to	Zn	at	54°	=	Zn	at	167°	to	Zn	at	54°	=	113	=	·1811
,, ...	Ni	,,	Pb	,,	58	=	Pb	,,	210	,,	Pb	,,	58	=	152	=	·0060
,, ...	Ag	,,	Ni	,,	56	=	Ni	,,	74	,,	Ni	,,	56	=	18	=	·0898
,, ...	Pb	,,	Pd	,,	58	=	Pd	,,	88	,,	Pd	,,	58	=	30	=	·1192
,, ...	Pd	,,	Fe	,,	60	=	Fe	,,	178	,,	Fe	,,	60	=	110	=	·0720
KF	Al	,,	Mg	,,	58	=	Mg	,,	82	,,	Mg	,,	58	=	24		
,, ...	Zn	,,	Al	,,	56	=	Al	,,	90	,,	Al	,,	56	=	34	=	·5083
,, ...	Cd	,,	Sn	,,	56	=	Sn	,,	120	,,	Sn	,,	56	=	64	=	·0651
,, ...	Cd	,,	Fe	,,	54	=	Fe	,,	194	,,	Fe	,,	54	=	140	=	·1235
,, ...	Pd	,,	Pt	,,	60	=	Pt	,,	202	,,	Pt	,,	60	=	142	=	·0209

* One leg of the bent tube was surrounded by a water-bath, which could be heated gradually.

Table XVII—(continued).

Solution.	Chemico-electric Positive.	Negative.	F.	Thermo-electric Positive.	F.	Negative.	F.	Difference in F. degrees.	Volt.
KBr	Al	Sn	58	Sn	184	Sn	58	126	·1456
,,	Cd	Al	58	Al	160	Al	58	102	·0979
,,	Al	Fe	60	Fe	105	Fe	60	45	·0616
,,	Sn	Ni	60	Ni	180	Ni	60	120	·3058
,,	Ag	Pd	60	Ag	180	Ag	60	120	·0814
KNO_3	Cd	Fe	52	Fe	140	Fe	52	88	·0634
,,	Pb	Sn	60	Sn	162	Sn	60	102	·1428
,,	Sn	Al	58	Al	72	Al	58	14	·0396
,,	Ni	Cu	54	Cu	90	Cu	54	36	·0785
,,	Cu	Ag	56	Ag	160	Ag	56	104	·0577
,,	Cu	Au	44	Au	200	Au	44	156	·1344
K_2CO_3	Mg	Al	56	Al	140	Al	56	84	
,,	Sn	Pb	56	Pb	190	Pb	56	134	
,,	Pb	Zn	56	Zn	108	Zn	56	52	
,,	Zn	Cd	58	Cd	168	Cd	58	110	
,,	*Pd	Au	40	Au	160	Au	40	120	·0562
,,	*Au	Pt	42	Pt	134	Pt	42	92	·0112
,,	*Pt	Ag	42	Ag	190	Ag	42	148	
Na_2HPO_4	Cd	Sn	56	Sn	120	Sn	56	64	·1595
,,	Sn	Al	56	Al	94	Al	56	38	·0666
,,	Al	Fe	56	Fe	145	Fe	56	89	
,,	Cu	Ni	56	Ni	170	Ni	56	114	
,,	*Pd	Pt	44	Pt	190	Pt	44	146	·0198
,,	*Pd	Au	42	Au	170	Au	42	128	·0481
KI	Pb	Al	56	Al	80	Al	56	24	·0605
NaCl	Al	Fe	56	Fe	202	Fe	56	146	·1261
,,	Fe	Sn	56	Sn	184	Sn	56	128	·1512
,,	*Pd	Pt	43	Pt	95	Pt	43	52	·0821
KCl	Cu	Ni	56	Ni	148	Ni	56	92	·06859
,,	Pb	Fe	56	Fe	74	Fe	56	18	
HCl	Au	Pt	56	Au	56	Au	134	78	·0792
K_2SO_4	Pb	Sn	50	Sn	173	Sn	50	123	·0809
,,	Al	Ni	58	Ni	202	Ni	58	144	·5135
,,	*Pb	Al	33	Al	162	Al	33	129	
,,	*Pd	Pt	40	Pd	40	Pd	60	20	·0109
KHO	Cd	Fe	54	Fe	180	Fe	54	126	·0056
,,	Fe	Cu	50	Cu	72	Cu	50	22	·4187
H_2SO_4	Sn	Pb	54	Pb	154	Pb	54	100	·1103
,,	Cd	Al	56	Al	182	Al	56	126	·1761
,,	Pb	Ni	50	Ni	164	Ni	50	114	·1729
,,	*Au	Pt	40	Pt	118	Pt	40	78	·0785
Na_2SO_4	Cd	Fe	56	Fe	192	Fe	56	136	·1613
,,	Pb	Sn	54	Sn	120	Sn	54	66	·0368
,,	*Ni	Cu	42	Cu	180	Cu	42	138	·0735
,,	*Ag	Pd	40	Ag	40	Ag	110	70	
,,	*Pt	Au	44	Au	180	Au	44	136	
,,	*Pd	Pt	42	Pd	42	Pd	134	92	·0742
HNO_3	Fe	Al	54	Al	108	Al	54	54	·2046
,,	Sn	Ni	50	Ni	190	Ni	50	140	·2439
,,	Ag	Pt	50	Ag	50	Ag	180	130	
$HClO_3$	Cd	Al	48	Al	144	Al	48	96	·1947
,,	Fe	Sn	50	Fe	50	Fe	176	126	·0126
,,	Sn	Pb	50	Sn	50	Sn	168	118	·0408
,,	Ag	Au	49	Ag	49	Ag	154	105	·0516
,,	*Pd	Pt	36	Pd	36	Pd	163	127	
$MgSO_4$	Zn	Al	48	Al	132	Al	48	84	·4420
,,	*Cu	Au	48	Cu	48	Cu	190	142	·0814
,,	*Pd	Pt	42	Pd	42	Pd	128	86	·0552
K Alum	Pb	Al	48	Al	120	Al	48	72	·0232
,,	Al	Sn	48	Sn	120	Sn	48	72	·0320
,,	*Au	Ag	38	Au	38	Au	100	62	
,,	*Pt	Pd	40	Pt	40	Pt	60	20	
Am Alum	Fe	Al	48	Al	102	Al	48	54	·0264
,,	Pb	Sn	52	Sn	98	Sn	52	46	·0478
,,	*Au	Pt	40	Au	40	Au	160	120	

Remarks.—The thermo currents produced by solutions of potassic carbonate and sodic phosphate were usually strong, and so also were those with aluminium, tin, and iron, and yielded many balances; whilst those from solutions of oxalic, formic, and tartaric acids were too feeble to balance the corresponding voltaic ones. In obtaining balances with the noble metals, the voltaic currents were so very small that the Thomson's reflecting galvanometer had to be employed; these cases are indicated by an asterisk.

By means of a separate experiment it was ascertained that the amount of error caused by the voltaic current producing polarisation before the thermo current had time to balance it was small, even in the cases where the former current was comparatively strong, as with a solution of potassic cyanide.

A number of thermo-electric reversals occurred during the making of these determinations: thus, in a solution of potassic nitrate, gold was thermo-electro-positive below and negative above 200 F., and platinum behaved similarly below and above 180°. Aluminium in solution of potassic carbonate became less thermo-positive above 140° F. Lead in sodic chloride and magnesium in potassic hydrate was thermo-positive below 170° and negative above it. In sodic sulphate nickel was thermo-negative below 110° and positive above that temperature. In dilute nitric acid lead was negative below and positive above 180°, and tin similarly below and above 150°. In dilute chloric acid iron was positive up to 150° and negative above it. In potassic alum nickel was positive below and feebly negative above 164° F. In ammonic alum aluminium was negative below and positive above 150°, and nickel was positive below and negative above 170° F. In magnesic sulphate cadmium behaved singularly; whilst it was chemico-negative to zinc and positive to aluminium at 60° F., by heating it alone (the zinc being in the cold part of the liquid) it became more negative, but with aluminium in the place of zinc, the cadmium became more positive by being heated. This experiment was repeated and similar results obtained; it is probably a case of the kind mentioned on pp. 279 and 288.

On comparing these reversals with the positions of the same metals in the same liquids in Table I, it will be observed that some of them do not agree in thermo-electric sign in the two cases. These apparent discrepancies may be partly accounted for by the circumstance that in the experiments of Table I, the metals were suddenly immersed in the hot and cold portions of liquid, whilst in the present ones they were immersed in the two cold portions, and one of them then gradually heated; they may also be partly explained by the circumstance observed that the balance obtained during a rising of temperature was frequently at a somewhat higher point than one obtained whilst the temperature was falling (compare also p. 272 for

effect upon reversal point by gradually heating the liquid). This difference of effect during a rising and falling of temperature is probably due to some hindrance to change of molecular movements analogous to that of supersaturation of a liquid by a salt.

A series of experiments were made to determine whether the difference in potential, caused in a voltaic cell by rise of temperature, is usually completely accounted for by the electromotive forces produced by the same rise of temperature of the same metals separately in the same liquids. The electric potentials in Table XVIII of the voltaic elements (A) at 60° F. (B) and 160° F. (C) were separately ascertained by means of the thermo-piles (p. 257); and those of the separate metals in the form of thermo-elements (D and E) as produced by a difference of from 60° to 160° F. were also separately determined in the same manner, and by the aid of the bent tube apparatus, &c. (see pp. 251 and 257). The liquids employed were of the same quality and strength as those mentioned on p. 252. The following table exhibits the results. The arrows represent the direction of the current at the heated junction:—

Table XVIII.

A Elements.	Potentials in volts of voltaic elements.			Potentials in volts of thermo-elements.		
	B At 60° F.	C At 160° F.	Difference.	D At 60° and 160° F.	E At 60° and 160° F.	Total potentials.
Neg. Pos.				Pos. Neg. Cu Soln.	Soln. Pt	
1. Pt and Cu in dilute solution of NaCl	·1760	·2106	·0346	→ ·044	no current	·0440
2. Pt „ Sn in dilute solution of KCl	·4769	·6533	·1764	Sn „ → ·0594	Soln. Pt → ·1244	·1838
3. Pt „ Pb in dilute $HClO_3$ ...	·5648	·5850	·0202	Pb „ → ·0201	„ „ → ·0317	·0518
4. Pt „ Sn in dilute solution of Am alum ...	·6008	·6740	·0732	Sn „ → ·0655	„ „ → ·0515	·1170
5. Cu „ Sn in ditto..............	·3735	·4796	·1061	Sn „ → ·0615	„ Cu → ·0381	·0996
6. Pt „ Sn in dilute HCl	·6998	·7726	·0728	Sn „ → ·1945	„ Pt → ·0100	·2045
7. Pt „ Sn in dilute H_2SO_4 ...	·4866	·8051	·3185	Sn „ → ·2912	„ „ → 0·216	·3128

Remarks.—These results show a considerable percentage of excess in four and a small one of deficiency in two of the cases; and indicate that the difference of potential produced in a voltaic cell by rise of temperature is not in all cases equal to the sum of the potentials produced by heating the two metals separately.

One of the objects of the research being to examine the relation of ordinary chemical corrosion to the thermo- and chemico-electric actions of metals in electrolytes, the rates of corrosion of as many as possible of the different metals employed (see Table I, p. 253), wholly immersed in an equal number of separate portions of the same liquid, were ascertained, with one liquid at a time, in order to compare the series thus arrived at with the orders of the same metals in the same liquid in the thermo- and chemico-electric series.

In carrying out this object, obstacles arose which prevented complete corrosion series being obtained to correspond with the thermo- and chemico-electric ones. In some cases the metals became covered with a film of gas, or with a solid coating, each of which altered the rate of corrosion; these cases were entirely rejected. In other cases the liquids contained traces of impurity which caused a solid film upon the metal; this was overcome by repeatedly immersing the metal in the liquid until the impurity was wholly removed, and then reimmersing it several times during longer periods, until concordant rates of corrosion were obtained. And in other instances the metal itself contained small amounts of impurity, which by exciting electric polarity, increased the corrosion. In addition to these and other influences, the rate of corrosion must have been affected by the unequal degrees of adhesion of the soluble products of corrosion to the corroding surface, and the unequal rates of diffusion of those substances into the surrounding liquid; these influences were reduced to a minimum by employing either very dilute solutions of the corroding agents, or such liquids as corroded but slowly. All these circumstances had to be considered, and each combination of metal and liquid treated in such a manner as to ensure the most attainably accurate result; but even under the most carefully prepared conditions, rate of corrosion is a very variable phenomenon.

With silver, palladium, gold, and platinum, it was necessary to use much larger sheets and stronger solutions, and to continue the expements during a longer period. The solutions of the two alums were four times, and those of potassic cyanide, sulphuric, hydrochloric, nitric, and chloric acids were of ten times, and the remainder of the usual strength. The silver was immersed in dilute nitric and chloric acids during one month and the other metals during three months. Most of the liquids corroded silver very slightly, and the metal acquired an adherent insoluble coating. The rates of loss are given in the table, but are separated from those in the weak liquids by horizontal lines.

The action was in no case allowed to continue longer than was sufficient to exhaust about 10 or 20 per cent. of the corroding substance. After excluding all doubtful cases, a sufficient number remained for the purposes intended. The rates of loss by corrosion

in all cases are given in grains per square inch per hour. The following are the orders of the degrees of rapidity of corrosion of the metals at 60° F.

Table XIX.—Simple Corrosion Series at 60° F.

	1 KCy.*		2 H_2SO_4.		3 HCl.		4 HNO_3.		5 KCl.		6 Am Alum.	
		Grain.		Grain.		Grain.		Grain.		Grain.		Grain
1......	Al	·0095	Mg	1·0000	Mg	·1581	Zn	·6130	Cd	·00036	Mg	·13770
2......	Zn	·0054	Fe	·0189	Zn	·0490	Cd	·3780	Zn	·00030	Zn	·00101
3......	Cu	·0053	Cd	·0026	Fe	·0118	Sn	·3750	Fe	·00026	Cd	·00066
4......	Ag	·0016	Pb	·0018	Cd	·0026	Mg	·1570	Sn	·00025	Pb	·00060
5......	Cd	·0014	Sn	·0009	Pb	·0023	Fe	·1406	Cu	·00012	Fe	·00059
6......	Pd	·0012	Cu	·0006	Cu	·0018	Pb	·0250	Al	·00006	Cu	·00038
7......	Au	·0009	Ni	·0005	Sn	·0017	Ni	·0030	Ni	Trace	Ni	·00025
8......	Sn	·0006	Al	·0004	Ni	·0004	Cu	·0008	Ag	Less	Sn	·00013
9......	Ni	·0005	Pd	None	Pd	None	Ag	·00022	Pd	None	Ag	·00000032
10......	Fe	Trace	Au	"	Au	"	Pd	None	Au	"	Au	None
11......	Pt	None	Pt	"	Pt	"	Au	"	Pt	"	Pd	"
12......	Pt	·00000036	...		...		Pt	"	...		Pt	"

	7 $HClO_3$.		8 KBr.		9 K Alum.		10 KI.		11 NaCl.		12 K_2SO_4.	
		Grain.		Grain.		Grain.		Grain.		Grain.		Grain.
1...	Mg	·3164	Cd	·00058	Mg	·0038	Zn	·00025	Fe	·00026	Pb	·00060
2..	Fe	·1068	Zn	·00050	Zn	·00047	Fe	·00020	Pb	·00012	Fe	·00048
3...	Sn	·0480	Cu	·00029	Fe	·00046	Cd	·00015	Cu	·00006	Zn	·00040
4...	Ni	·0079	Fe	·00026	Cd	·00041	Ag	·00005	Sn	·00005	Sn	·00036
5...	Pb	·0074	Pb	·00022	Ni	·00022	Sn	Trace	Ni	·00001	Cd	·00028
6...	Cd	·0040	Al	·00010	Cu	·00018	Ni	Less	Ag	Trace	Ag	None
7...	Cu	·0008	Ni	None	Sn	·00005	Pd	None	Au	None	Au	"
8...	Ag	·000026	Au	None	Ag	·00000032	Au	"	Pd	"	Pd	"
9...	Au	None	Pd	"	Pd	None	Pt	"	Pt	"	Pt	"
10...	Pd	"	Pt	"	Au	"	...		...		...	
11...	Pt	"	...		Pt	"	...		...		...	

Remarks.—These series and numbers show, 1st, that the order of rapidity of corrosion differed with every different metal and in every different solution; 2nd, the rates of corrosion of each metal varied in nearly every separate liquid; 3rd, in each solution also it differed with every different metal; 4th, the most chemically positive metals were usually the most quickly corroded; and 5th, the corrosion of metals was usually the fastest in the most acid liquids. These results indicate that the rate of corrosion depends both upon the metal and upon the liquid, probably most upon the former. With regard to the effect of chemical composition of the liquid, the differences of order between the series with a solution of potassic cyanide and with

* This sample was found by analysis to contain 89·14 per cent. of actual cyanide.

dilute sulphuric acid at 60° F. were found to be greater than those between the series with potassic bromide and chloride.

Comparisons of the order of the corrosion series at 60° F., with the thermo-electric ones of the same liquids of Table I (p. 253), and the chemico-electric ones of the "cold" columns in Table X (p. 265) show that the order of simple corrosion differs largely from that of chemico-electric, and still more largely from that of thermo-electric action. In no one pair of the twelve corresponding series of Tables X and XVIII did the order of metals entirely agree, the least agreeing ones were those in potassic cyanide and bromide. The degree of chemico- or thermo-electric potential therefore does not vary in the same order as that of rapidity of simple corrosion in any of the liquids examined. These different degrees of relative divergence of order of the three tables indicate a greater degree of complexity of thermo-electric than of chemico-electric action of metals in liquids in the given cases, and a less dependence of the former than of the latter upon simple corrosion. As real thermo-electric action of metals in liquids does not necessarily include corrosion, the latter when it occurs with the former is not the cause of it.

With regard to the relation of voltaic action to corrosion, Poggendorff published ("Phil. Mag.," 1840, vol. xvi, pp. 495 and 537) a number of experiments showing that the chemico-electromotive force was not the strongest in those liquids where the corrosion of the metal was apparently the most rapid, but he does not appear to have actually verified the relative losses of weight by weighing the metals; nor to have excluded from his tables those metals which became coated with a film of insoluble matter.

In order to ascertain whether rise of temperature caused every metal to corrode faster, or had always the effect of making only those metals corrode faster which by it were rendered more chemico-electro-positive, and those more slowly which were rendered negative, series of corrosion experiments were made with most of the same metals and liquids (see p. 252) at 160° F. as were employed in the thermo-electric experiments of Table I, care been taken to keep the metals wholly immersed, and the liquids from altering in strength by evaporation. The length of time during which it was necessary to continue the experiment varied from five minutes to eight hours. Table XX (p. 286), shows the rates of loss in grains per square inch per hour. The proportionate rates of increase for 100° F. rise of temperature, as calculated from the numbers on Table XIX, p. 284, are also given for comparison.

Remarks.—By comparing these results with those obtained at 60° F., as shown in Table XIX, p. 284, it will be seen that in nearly every case rise of temperature increased the rate of corrosion. The only exception in the total fifty-seven cases was zinc in dilute nitric acid,

Table XX.—Simple Corrosion Series at 160° F.

	KCy.			HCl.			$HClO_3$.			Am Alum.		
			Increase from 1 to—			Increase from 1 to—			Increase from 1 to—			Increase from 1 to—
1.........	Al	·4940	52	Mg	1·2106	7·65	Mg	1·4507	4·58	Mg	·3520	2·55
2.........	Cu	·1258	23·7	Zn	·2090	4·26	Fe	·6140	5·84	Fe	·0732	124·00
3.........	Zn	·0495	9·1	Fe	·1370	11·6	Cu	·2573	321·6	Zn	·0382	37·80
4.........	Au	·0282	31·3	Al	·0273	...	Zn	·2160	...	Pb	·0138	23·00
5.........	Ag	·0070	4 4	Pb	·0231	10·0	Pb	·1940	26·20	Cd	·0073	11·00
6.........	Pd	·0051	4·2	Cu	·0156	8·66	Sn	·1690	3·52	Al	·0071	...
7.........	Cd	·0046	3·3	Cd	·0096	3·7	Cd	·1070	26·70	Sn	·0060	46·10
8.........	Sn	·0042	7·0	Sn	·0078	4·6	Ni	·0474	6·00	Cu	·0032	8·42
9.........	Pb	·0031	...	Ni	·0039	9·7	Al	·0215	...	Ni	·0014	5·60
10.........	Ni	·0027	5·4	Ag	·0026	...	Pt	·00015	...	Ag	Trace	...
11.........	Fe	·0010	...		...	...		...	...		...	...
12.........	Pt	None	...		...	...		...	...		...	...
	Average...		20·0	Average...		7·52	Average...		56·35	Average...		32·31

	HNO_3.			H_2SO_4.			K Alum.		
			Increase from 1 to—			Increase from 1 to—			Increase from 1 to—
1......................	Mg	·8853	5·64	Mg	4·0426	4·04	Mg	·1226	32·21
2......................	Cd	·8176	2·16	Zn	·2510	...	Fe	·0821	178·00
3......................	Zn	·6040	·98*	Fe	·2493	13·20	Zn	·0386	82·10
4......................	Fe	·6000	4·26	Al	·0284	71·00	Pb	·0138	...
5......................	Sn	·5666	1·51	Pb	·0196	10·90	Al	·0090	...
6......................	Pb	·2155	8·62	Sn	·0115	12·80	Sn	·0080	160·00
7......................	Al	·0260	...	Cd	·0091	3·50	Cd	·0077	18·80
8......................	Ni	·0151	5·03	Cu	·0062	10·30	Cu	·0028	15·55
9......................	Cu	·0133	16·60	Ni	·0057	11·40	Ni	·0022	10·00
	Average...		5·60	Average...		17·14	Average...		70·95

and this was a very feeble one. The increases of rate of corrosion under the influence of heat were extremely variable, whilst that of tin in dilute nitric acid was increased 1·5 times, that of copper in hydrochloric acid was increased 321·6 times; both these metals and acids were very pure, and not a trace of oxide appeared upon the tin. The total average increase for the entire series was 29·98. The changes produced in the orders of the series by difference of temperature were least in dilute hydrochloric acid. The results contained in Tables XIX and XX show that when metals are rendered thermo-electro-negative in liquids by being heated, their electric change is not attended by diminished rapidity of corrosion. It was found by comparing Table XIX with Table I that the proportion of cases in which the metal most corroded was more thermo-

* This was the only case of decrease, and was verified by means of a repetition experiment.

positive or less negative to that in which it was less positive or more negative, was 384 to 199 (= 65·87 per cent.), or as 1·93 to 1; also by similarly comparing Table XX, that the proportion in which the metal most corroded at 160° F. was most positive or less negative to that in which it was the reverse, was 338 to 151 (= 69·12 per cent.), or as 2·24 to 1.

And with regard to the relation of corrosion to voltaic state, it has been assumed "that in all these cases the positive electricity sets out from the more oxidable metal, and traverses the liquid towards the less oxidable one;" but comparisons of Tables X, XIX, and XX show that this is not entirely correct; with twelve different liquids at 60° F. the most chemico-electro-positive metal was also the most corroded one in 337 cases, and the least corroded in 62=a proportion of 5·43 to 1, or 84·44 per cent.; and with seven liquids at 160° F. the numbers were respectively 248 and 59=a proportion of 4·20 to 1, or 80·77 per cent. Neither volta-positive nor negative state, therefore, is always proportionate to degree of ordinary corrosion, either in cold or hot liquids. These figures show also that by a rise of temperature of 100° F. the proportion of cases in which the negative metal was the most corroded, increased from 15·56 to 19·23 per cent. of the entire number of cases. Comparison of the above proportions with those of the relation of corrosion to the thermo-electric states of metals in liquids, viz., 1·93 to 1 and 2·24 to 1 (see above), shows that corrosion usually influenced in a much greater degree the chemico- than the thermo-electric properties of such combinations.

Not only was the relative number of cases in which the volta-negative metal was the most corroded increased by rise of temperature, but also the average relative loss by corrosion of the negative one to that of the positive was more than doubled.

The following appear to be cases in which contact influenced the thermo-electric currents of metals in liquids. With each instance, in the first part of the experiment the metal was suddenly immersed in the hot liquid; and, in the second part, the same metal and liquid were gradually heated. In a solution of potassic bromide, copper alone was thermo-positive, but when gradually heated in contact with tin it was thermo-negative; in one of potassic nitrate platinum alone was thermo-positive, but in contact with gold it was rendered slightly more negative during a slight elevation only of temperature; in potassic hydrate magnesium alone was thermo-negative, but in contact with lead it was rendered more positive by heat up to 170° F.; in sodic sulphate nickel alone was thermo-positive, but in contact with tin it was more negative up to 110° F.; in dilute nitric acid lead alone was thermo-positive, but in contact with iron it was more negative up to 180° F.; in the same liquid tin alone was thermo-negative, but in contact with nickel it was more positive up to 160° F.;

and in dilute chloric acid iron alone was thermo-negative, but in contact with tin it was more positive up to 150° F. These cases were observed whilst making experiments to determine the balance points of Table XVII (p. 279), but were not included in that table because they did not yield balances.

In each of these causes the electric effect of heat upon the same metal in the same liquid was opposite to its usual thermo-electric one in that liquid. In each case also its effect was to increase, within the limits of a moderate rise of temperature, the strength of the already existing voltaic current. These changes may be explicable by supposing that the additional irregularities of electric potential introduced by the second metal at its two points of contact with the liquid and original metal altered the electric condition of the subsequently heated metal. The pre-existing voltaic currents also were so feeble that these unusual effects of heat do not appear to have been due to chemical changes of the liquid at the heated electrode; such changes, and the degrees of polarisation due to them, must also have been small in comparison with those already produced by ordinary chemical corrosion.

In a separate communication "On some Relations of Chemical Corrosion to Voltaic Currents," "Proc. Roy. Soc.," vol. 36, p. 331, I have examined the amounts of external voltaic current produced by the corrosion of known weights of various metals at atmospheric temperature.

The general conclusions drawn from the experiments described in the present paper may be found in the "Abstract," "Proc. Roy. Soc.," vol. 36, p. 50.

The fact that the mere contact of the metal and liquid of a thermo-electric pair is attended by electric polarity, and that either heating the one junction or cooling the other of such a couple produces an electric current, may be regarded as evidence that at all times, whilst no heat or cold is being applied to either of these joinings, the molecular actions at the two points of union tend to produce a current, but as these tendencies are opposite in direction and equal in strength, no current occurs. Each of these junctions may also be regarded as a surface of contact previously possessing a tendency to alter in temperature, and this view is supported by the discovery of Pouillet, that even when non-corrodible metals touch liquids, a slight change of temperature occurs ("Annal. de Chimie," 1822, pp. 141–162).

In support of this view, it may be observed that amongst the instances composing the chemico-electric series of Table X are many the currents of which are at the most of doubtful chemical origin. Of these, are the cases in which currents are produced by non-corroded metals, such as platinum, gold, palladium, and silver, in feebly corrosive liquids, including solutions of neutral salts. A suitable

instance of this kind is silver and platinum in a strong solution of potassic fluoride. The electromotive force of this combination was found to be ·0477 of a volt (see Table XII, p. 273).

In order to test the inference "that at all times, whilst no heat or cold is being applied to either of the junctions of the thermo-electric couple, the molecular actions at the two points of union tend to produce a current," I suspended, by means of a pure silver wire, a sheet of pure silver, $2\frac{1}{2}$ inches wide and 3 inches deep, between two sheets of platinum, 3 inches wide and 6 inches deep, in a glass cell, containing a solution of pure potassic hydrate, 10 grains per ounce of water, the platinum being immersed 4 inches, and the silver wholly, in the liquid, and the two metals united outside by the wire. At the outset, the platinum was positive to the silver, about 5° deflection being produced of the needle of a galvanometer of 100 ohms resistance, and after a few hours 1°, and permanently half a degree; it then remained constant at that point, as shown by occasional testing during about four months, when, by concentration of the liquid by evaporation, the current was feebly reversed. After diluting the liquid to its original strength, the platinum was again positive half a degree, and continued so during a further period of one year. In a similar previous experiment with smaller plates similar results were obtained.

To ascertain whether the platinum was corroded, two electrodes—one of sheet silver and one of platinum—each about 1 inch wide and 3 inches deep, were partly immersed in a similar solution of the hydrate, and a current from two Smee's cells passed through the liquid, the platinum being the anode, during several weeks. The plates were then reweighed; the platinum had not perceptibly diminished in weight, and the silver had lost less than ·01 grain.

As an additional example, when gold and platinum produce an electric current in a solution of neutral potassic sulphate, gold being positive (see Table X, p. 266), the solution does not corrode them (see p. 283), and the current is therefore not due to chemical corrosion.

In each of such cases there are three points of heterogeneous contact:—1st. Where the liquid A touches the metal B; 2nd. Where it touches the metal C; and 3rd. Where the metals B and C touch each other. At each of these points the substances are rendered electro-polar by molecular action, and as a feeble current occurs the polarities thus produced do not balance each other. As also a less degree of electric potential is necessary to make a current pass through a suitable electrolyte than to cause a refractory metal such as gold or platinum to corrode, the current is produced without corrosion of metal. This current at once begins to produce the usual effect of raising obstacles to its own continuance, by causing polarisation at the electrodes, and becomes more feeble. As a portion of the current, however, continues permanently, producing the usual effect

upon a galvanometer needle, a permanent source of power must exist; and as corrosion of metal does not occur, this source of power must be different from that of the voltaic current; such currents have been attributed to capillary action.

"Report to the Solar Physics Committee on a Comparison between apparent Inequalities of Short Period in Sun-spot Areas and in Diurnal Temperature-ranges at Toronto and Kew." By BALFOUR STEWART, M.A., LL.D., F.R.S., and WILLIAM LANT CARPENTER, B.A., B.Sc. Communicated to the Royal Society at the request of the Solar Physics Committee. Received April 21. Read May 1, 1884.

1. It has been known for some time, through the researches of Sabine and others, that there is a close connexion between the Inequalities in the state of the sun's surface as denoted by sun-spot areas and those in terrestrial magnetism as denoted by the diurnal ranges of oscillation of the declination magnet; and moreover, the observations of Baxendell, Meldrum, and various other meteorologists have induced us to suspect that there may likewise be a connexion between solar inequalities and those in terrestrial meteorology.

This latter connexion, however, assuming it to exist, is not so well established as the former—at least if we compare together Inequalities of long period. Attempts have been made to explain this by imagining that for long periods the state of the atmosphere, as regards absorption, may change in such a manner as to diminish or even cloak the effects of solar variation by increasing the absorption when the sun is strongest and diminishing the absorption when the sun is weakest.

On this account it has seemed to us desirable to make a comparison of this kind between short-period Inequalities, since for these the length of period could not so easily be deemed sufficient to produce a great alteration of the above nature in the state of the atmosphere.

The meteorological element which we have selected for comparison with sun-spots has been the diurnal range of atmospheric temperature—an element which presents in its variations a very strong analogy to diurnal declination-range.

2. There are two ways in which a comparison might be made between solar and terrestrial Inequalities. We might take each individual variation in sun-spot areas and find the value of the terrestrial element corresponding in time to the maximum and the minimum of the solar wave. If we were to perform this operation for every individual solar Inequality and add together the results, we might

perhaps find that the magnetic declination-range was largest when there were most sun-spots. If, however, we were to make a similar comparison between sun-spot daily areas, that is to say, the daily aggregate areas occupied by spots including penumbræ, and diurnal temperature-ranges, we might not obtain a decisive result. For at certain stations such as Toronto, it is suspected (the verification or disproval of this suspicion being one of the objects of this paper) that there are two maxima and two minima of temperature-range for one of sun-spots. This is not a phenomenon unknown in other branches of meteorology, as for instance in barometric pressure, which has two oscillations in one day. Its effect, however, might be such that, in a comparison made as above, the temperature-range corresponding to a maximum of sun-spots might be equal in value to that corresponding to a minimum, or, in other words, we should get no apparent result, while, however, by some different process, proofs of a real connexion might be obtained.

But if we can get evidences of apparent periodicity in sun-spot fluctuations when dealt with in a particular manner, we have a method which will afford us a definite means of comparison. And here, as Professor Stokes has pointed out, it is not necessary for our present purpose to discuss the question whether these sun-spot Inequalities have a *real* or only an *apparent* periodicity.* All that is needful is to treat the terrestrial phenomena in a similar manner, or in a manner as nearly similar as our observations will allow, and then see whether they likewise exhibit periodicities (apparent or real) having virtually the same times as those of the sun-spots—the phases of the two sets of phenomena being likewise allied in a constant manner. It is such a comparison that we have here made, but before describing the results obtained, it will first be necessary to describe the method of analysis which we have adopted.

Method of Analysis.

3. Our method is one which enables us to detect the existence of unknown Inequalities having apparent periodicity in a mass of

* It is well known that when we discuss a series of figures, we frequently detect peculiarities of some kind, which strike the eye but have no scientific meaning. Such peculiarities might occasionally assume the form of apparent periodicities, and there is no reason why they should not manifest themselves when the figures we are discussing represent the progress of some natural phenomenon. Moreover, in this last case we may sometimes perceive irregular attempts at repetition, such as we observe in volcanic phenomena, to which, however, we are unable to assign any true periodicity. Now it is imaginable that such appearances as we have indicated above may take place in our list of sun-spot areas without implying the presence of true periodicity; but whether such marks of periodicity as those records present be real or only apparent, *inasmuch as they do occur* we should expect them likewise to occur in such terrestrial phenomena as are closely connected with sun-spots.

observations. A description of this method has already been published in the "Proceedings of the Royal Society," May 15, 1879, but in order to render what follows more clear, we shall to some extent reproduce this description.

Imagine, by way of illustration, that we have in our possession a long series of temperatures of the earth's atmosphere at some place in middle latitudes, and that, independently of our astronomical knowledge, we were to make use of these for the purpose of investigating the natural Inequalities of terrestrial temperature. We should begin by grouping the observations according to various periods taken, say, at small but definite time-intervals from each other. Now, if our series of observations were sufficiently extensive, and if some one of our various groupings together of this series should correspond to a real Inequality, we should expect it to exhibit a well-defined and prominent fluctuation, whose departure above and below the mean should be of considerable amount. Suppose, for instance, that we have 24 places in our series, that is to say, we proceed to group our mass of temperature observations in rows of 24 each, and suppose further that the time-interval between two contiguous members of one row is exactly one hour. The series would thus represent the mean solar day, and we should, without doubt, obtain from a final summation and average of our rows, a result exhibiting a prominent temperature fluctuation of a well-defined character, which we might measure (as long as we kept to 24 points) by simply adding together all the departures of its various points from the mean, whether these points be above or below; in fine, by obtaining the area of the curve which is the graphical representation of the Inequality above and below the line of abscissæ taken to represent the mean of all the points.

4. Suppose next that, still keeping to rows of 24 places, we should make the time-interval between two contiguous members of a row somewhat different from one hour, whether greater or less, we should now in either case obtain a result exhibiting, when measured as above, a much smaller Inequality than that given when the interval was exactly one hour; and it is even possible that if our series of observations were sufficiently extensive we should obtain hardly any traces of an Inequality whatever. In fine, when each row accurately represented a solar day, the result would be an Inequality of large amount, but when each row represented a period either slightly less or greater than a day, the result would be an Inequality of small amount.

This process as far as we have described it is not new, having been already used by Baxendell and probably by other observers of stellar variability. In the present instance we should by its means, after bestowing enormous labour in variously grouping in accordance with

a great number of periods taken at small intervals from each other, obtain at length an accurate value of the mean solar day.

5. Reflection will, however, convince us that it is possible greatly to abridge this labour. Let us still keep to our illustration of temperature observations but, since ignorance regarding the length of the day is inconceivable, let us imagine that a staff of observers have strict orders to register the temperature at every exact hour of a mean time clock. Let us further suppose that the rate of this clock is constant but not quite correct, and that the observations so made extend over a series of years. It is required by means of these observations to determine the clock-rate.

To do this we might, if we chose, laboriously arrange the whole body of observations in a number of different groups, one group representing 24 hours by the clock, and other groups a little more or a little less. Amongst these some one grouping would be found to exhibit when the summations were made a maximum amount of fluctuation, and we should naturally imagine that the time-scale of this grouping was the best representative of the mean solar day.

We might, however, obtain the same result with infinitely less labour, and for this purpose we need not make any other fundamental grouping than that which denotes 24 clock hours.

Let us then arrange these observations in yearly series and find the average Inequality for each year.

Now suppose that for the first year the mean maximum diurnal temperature occurred at 2 P.M. clock time, for the second year at 1 P.M., for the third year at noon, and so on, retiring backwards one hour every year. We should, of course, conclude that the clock went one hour slow every year, and should thus obtain at once the clock-rate. And by pushing the second year's Inequality one place forward, the third year's two places forward, and so on, adding together the series so placed, and ultimately taking the mean, we should obtain a good value of the true temperature Inequality corresponding to the exact mean solar day.

We have chosen a very obvious instance, but of course the clock error in one year might not be exactly one hour, and it might be necessary to arrange the yearly series in a number of different ways with regard to each other before we obtained the grouping which gave us the maximum Inequality. But at length this result would be obtained and without the expenditure of more than a moderate amount of labour, and when obtained the time-scale belonging to it would represent the mean solar day.

It is virtually this method which we have employed in our present investigation, and we shall now state what observations, solar and terrestrial, we have at our disposal, and also how we have applied our method to these observations.

Sun-spot Observations and their Treatment.

6. We have begun by taking daily values of sun-spot areas commencing with Schwabe's observations, which have been reduced by Messrs. De La Rue, Stewart, and Loewy. These reduced records giving total areas for individual days will be found in an Appendix to the Report of the Solar Physics Committee. They extend from 1832, the date when Schwabe appears to have matured his system of observation, to 1853 inclusive. From 1854 to 1860 inclusive, we have made use of Carrington's accurate observations; for 1861 we have had to fall back once more upon Schwabe; while from 1862 to 1867 inclusive we have the Kew series of observations.

These various series form, when put together, one whole series of 36 years. In many cases, owing to bad weather, there are gaps or blanks between observations which we have filled up by the simplest possible form of interpolation. These 36 years' observations, when the blanks have been filled up, give us therefore one long series of more or less trustworthy daily records of the total area of spots on the sun's visible disk, the unit of measurement being the one-millionth of the sun's visible hemisphere.

7. Let us now in the first place group together these sun-spot observations in rows of 24 days each, dividing the whole body of observations so tabulated into 36 yearly series. A year will contain generally 15, but occasionally 16 lines of 24 places each, that is to say, it will consist of 15, or sometimes 16, horizontal rows, the number of places in each horizontal row being always 24. The sums of each of the 24 vertical columns for each year are now taken. We have thus for each year a series of 24 sums. But it is obvious that these sums will be greater in years of maximum than in years of minimum sun-spots, so that to use them as they stand would virtually mean to give great weight to years of maximum as compared with years of minimum sun-spots in our search for Inequalities. We have endeavoured to overcome this difficulty in the following manner. If sun-spot observations be put into a graphical form, it will at once be seen that whenever the total sun-spot area is great, the oscillations of this, or ranges of sun-spot Inequalities, are great also, and that these ranges may, as a first approximation at least, be deemed proportional to the general sun-spot activity. In other words, the oscillations of areas are nearly proportional to the areas. We have, therefore, taken the above-mentioned 24 numbers for every year, and proportionally altered them so that the sum of the 24 shall for each year be equal to 24,000. Each sum for each year should, therefore, be equal to 1000, provided it be not subject to the influence of some Inequality.

If, however, there be a 24-day Inequality, some of these values will be larger and others smaller than 1000, and it is their differences from 1000 that we finally record with *plus* and *minus* signs in order to form our yearly series. It is, however, necessary here to remark that in certain minimum years the observations which exhibit sun-spots are sometimes too unfrequent to render the year tabulated as above in rows of 24 a trustworthy representative of the Inequalities we are in search of. In such cases we have taken in addition half a year on each side of the minimum year, so that we have thus the mean of two years instead of one, the central point of the series being, however, as before, the middle of the minimum year. This treatment has been applied to the years 1833, 1844, 1855, and 1856.

The course of procedure described above may, we think, be justified by the following considerations:—

We are engaged in applying to the sun-spot records a method of analysis unquestionably founded on repetition. We are, therefore, in as far as our analysis is concerned, trying to find if there is repetition in solar records, that is to say, trying to discover if they are possessed of periodicity, whether apparent or real. Then the question arises how to prevent years of many sun-spots from dominating in our results to the exclusion of years of few spots. Now it is nearly certain that, assuming a connexion between the state of the sun and variations in terrestrial meteorology, sun-spots are only a celestial *symptom* of a certain variable state of the sun's surface, perhaps of a state of variable power, which is the true cause of terrestrial variability. That is to say, it is highly probable that spots are not themselves the causes of these terrestrial changes, and it is quite certain that the *efficient* (in contradistinction to the *apparent sun-spot*) *variation* in the state of the sun, whatever it be, will not be found to present ranges so great as those presented by sun-spots; for instance, spots may be entirely absent from the sun's surface, and yet the sun will cause diurnal variations in magnetism and meteorology of large amount. An *efficient* in contradistinction to a *sun-spot* oscillation of short period (presuming this to exist), may perhaps vary in magnitude from times of maximum to times of minimum sun-spots—of this we are ignorant—all that we know is that the variation in magnitude of this efficient oscillation is doubtless less than that of the sun-spots which indicate it. The efficient oscillation may possibly be represented by a periodic function of short periodicity having a coefficient which varies from times of maximum to times of minimum sun-spots. But if so we are at present concerned only with the short period function, and we can see no other way of obtaining this than by assigning what we imagine to be an average value to the coefficient of the indicating sun-spot variation so as to give all years an equal weight. This is virtually what we have done in our

treatment of sun-spots as above described, and we have adopted a similar plan with our terrestrial records.

8. Thus at length by the means now described we obtain 36 yearly series corresponding to the years from 1832 to 1867 inclusive, each series containing positive and negative quantities whose sums are exactly equal. Let us represent these as follows:—

$$
\begin{array}{ccccccc}
(1)_{32} & (2)_{32} & (3)_{32} & . & . & . & (24)_{32} \\
(1)_{33} & (2)_{33} & (3)_{33} & . & . & . & (24)_{33} \\
. & . & . & . & . & . & \\
. & . & . & . & . & . & \\
. & . & . & . & . & . & \\
(1)_{67} & (2)_{67} & (3)_{67} & . & . & . & (24)_{67}
\end{array}
$$

Now if we allow these terms to stand as above and take the sums of the various vertical columns, we shall obtain a result denoting (when divided by 36) the final Inequality corresponding to 24 days exactly as well as this can be represented by the whole body of observations at our disposal. Suppose, however, we arrange the horizontal lines in such a manner that $(24)_{33}$ comes under $(23)_{32}$, and $(1)_{34}$ under $(24)_{33}$, and so on. If we then sum up the vertical lines so arranged, the result will now represent an Inequality having a period so much longer than 24 days, that the difference amounts to one day in the course of a year. This period will easily be found by the following proportion. If in 365·25 days things advance 1 day, how much will they advance in 24 days? The result is $\frac{24}{365{\cdot}25}=0{\cdot}0657$, and hence the period of the Inequality in question will be 24·0657 days. In the above example things have been pushed each year one place backwards, that is to say, from right to left; had they been pushed one place forwards this would have denoted an Inequality as much less in period than 24 days, as the one we have now considered is greater, in other words, one with a period of 23·9343 days. So in like manner two or three places backwards each year will denote Inequalities with periods of 24·1314 and 24·1971 days, and two or three places forwards inequalities with periods of 23·8686 and 23·8029 days and so on. Here the law is sufficiently obvious. The comparative magnitude of the final Inequality so obtained may be found by simply adding together all its 24 terms without respect of sign, the sum representing the total area of departure from the mean, which the Inequality would have exhibited had it been put into a graphical form.

9. To obtain the proper phase of the Inequality let us arrange our series of individual observations so that the first begins with January 1, 1832, and let us suppose, for instance, that we are dealing

with an Inequality which exceeds the 24-day period by six days in one year. Our object is to represent this and all other Inequalities with the phase they have in the beginning of 1832, that is to say, to represent each by a row of 24 figures (plus and minus), the first of which denotes the phase on January 1, 1832. Now, since the particular Inequality in question is larger in period than 24 days, corresponding phases will gradually advance to the right, and hence the *mean* phase for 1832, which is denoted by the *true* phase for July 1 of that year, will be three (*i.e.*, one-half of six) divisions to the right as regards the phase for January 1. Hence it will be necessary, in order to represent this Inequality as it was on January 1, 1832, to push the 1832 series three places to the left, and then each successive yearly series six places to the left of the one before it. The method of setting for a difference representing an exact number of days in each year is thus obvious.

It is equally easy to set for differences representing not only whole days, but also fractional parts of days. For instance, let it be required to set for an Inequality greater than 24 days by 3·75 days each year. Bearing in mind from what we have said that the mean of 1832 is $\frac{3{\cdot}75}{2}$ days wrong, we have as follows:—

Year.	Phase has gone to right.	Phase has to be pulled to left (nearest whole number).
1832	1·875	2
1833	5·625	6
1834	9·375	9
1835	13·125	13
1836	16·875	17
	and so on.	and so on.

10. Having thus explained our principle of setting we shall now briefly describe the practice which it has been found most convenient to adopt. The whole number of yearly series is regarded as being made up of three sets of 12 years each, namely, 1832–43, 1844–55, 1856–67, and Inequalities are obtained representing 1, 2, 3, 4, &c., days' difference *plus* or *minus* in 12 years. Thus 24_{-39} or simply —39 represents, according to our notation, an Inequality whose period falls short of 24 days by 39 days in 12 years, and so on.

It has not been deemed necessary to make separate and complete settings for each of the small stages indicated above, but only for every third. Thus, for instance, we should make separate and complete settings for —42, —39, —36, &c., obtaining in each case three partial sums of 12 years each, the sum of which would represent the final Inequality. But with regard to the Inequality —41 we should esteem it sufficiently accurate if the first set of 12 years for —42 be

kept as it is—the second set be pushed to the left of the first one division, and the third to the left of the second one division—the sum of the three 12-yearly series so placed being taken to represent -41. In like manner for -43 we should push the second 12-yearly series of -42 one division to the right of the first, and the third one division to the right of the second.

The method which we have now described applies both to the Inequalities around 24 days of which we have spoken, and to those around 26 days which we are also about to exhibit. Here, however, one day in a year means $\frac{26}{365 \cdot 25}$ or ·0712 day. In other respects the treatment is the same for both.

Temperature-range Observations and their Treatment.

11. We are indebted to the Meteorological Council, the Kew Committee, and to Messrs. Kingston and Carpmael, the late and the present Directors of the Toronto Observatory, for 36 years of the Diurnal Temperature-ranges at Toronto, extending from the beginning of 1844 to the end of 1879. We have also obtained 24 years of the Diurnal Temperature-ranges at Kew, extending from 1856 to 1879, for which we are indebted to the Kew Committee. These observations have been split up into series of 12 years each, the first two series of Toronto and the last series of Kew being coterminous with solar series, the last series of Toronto and the last of Kew being, however, 12 years later than the last solar series.

It thus appears that a different series of years is represented in each case. It would, of course, have been better for our purpose if precisely the same series of years had been represented by the three sets of observations; but as this is not the case we have not scrupled to make the comparison implied in this research between the whole body of observations as they stand, believing that in so doing we shall make the best use of the materials placed at our disposal.

12. Here it will be necessary to allude to a method of treatment which we have thought it desirable to apply to the temperature-range observations, but which we have not found it necessary to apply to those of sun-spots.

Having obtained the yearly Inequalities corresponding to 24 days in exactly the manner already described, we have to some extent smoothed or equalised our series of differences. The primary series (A) has been converted into another series (B), also of 24 sums, each sum of (B) being the mean of four consecutive sums of (A), and the series (B) has been converted by a similar process into a series (C), each sum of which is the mean of four consecutive sums of (B). It is the series (C) which is ultimately made use of.

The temperature-range Inequalities around 26 days have been dealt with in a similar manner, except that (B) is a mean of 3 not 4 sums of (A), and (C) a mean of 3 not 4 sums of (B). Here, too, it is the series (C) which is ultimately used.

13. These smoothed series have then been treated exactly after the manner in which we have treated the sun-spot series, and with them too the first term of each Inequality represents the phase of that Inequality which would have corresponded to January 1, 1832, provided the terrestrial records had extended back to that date. Thus the phases of the celestial and terrestrial Inequalities both start from the same epoch, and are in this respect strictly comparable with one another.

Limits of Accuracy of this Method.

14. It is obvious that the method now described can only be considered as correct for periods not far removed on either side from that for which the series was originally framed. Thus in the case of 24 days it is apparent that 12 places pushed backwards every year would imply the same setting as 12 places pushed forwards, and also for 26 days 13 places pushed backwards each year would yield the same result as 13 places pushed forwards; it will not, therefore, do to extend the method nearly so far. It is imagined that four days a year on each side will form a legitimate boundary, or, adopting the notation of Art. 10, that the region between the limits −48 and +48 will not be seriously open to objection. Considering it, however, desirable that this point should be verified by some kind of actual trial, we have adopted the results deduced from a series of observations of Diurnal Declination-ranges at Kew excluding disturbances ("Proc. Roy. Soc.," May 15, 1879). These observations extend from 1858 to 1873 inclusive, forming a series of 16 years, and they have been arranged as follows:—

α. In yearly series representing 24 days in length, beginning January 1, 1858.

β. In yearly series representing 24·25 days in length, beginning January 1, 1858.

Both of these series have been treated according to the method described above, and settings have been made at distances denoting half a day in a year, and extended on each side to lengths which are probably beyond the limits of safety. The magnitudes of the Inequalities represented by the various settings have been obtained by summing up the positive and negative departures, and the results are exhibited graphically in the following diagram in which abscissæ represent periods, while the ordinates represent corresponding Inequalities.

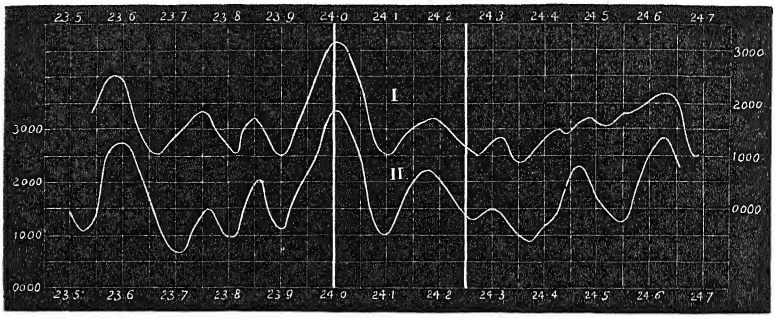


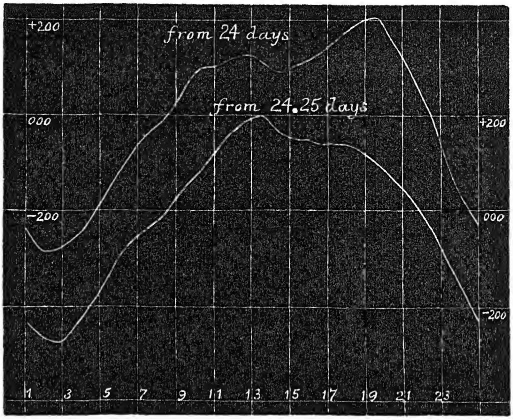
Fig. I. Kew Declination-range, 24·25 days.
Fig. II. Kew Declination-range, 24·00 days.

It will at once be seen by comparing figs. I and II that they both exhibit very nearly the same positions for maximum Inequalities. Towards the right, however, there are indications that the limits of safety have been exceeded. But there seems reason to suppose that we shall be safe within the limits which we have set for ourselves, and which correspond to about a quarter of a day on either side of the normal.

Our next point is to ascertain how far the phase of a prominent Inequality, as, for instance, that near 24 days determined by the one series, agrees with the same as determined by the other. It will, we think, be seen from the following table, as well as from its graphical representation which accompanies it, that there is a fairly good agreement in phase between the two values of the Inequality so determined.

Table I.—Exhibiting the Inequality at 24 Days as Determined by 24_{-0} Days and $24{\cdot}25_{-44{\cdot}4}$ Days.

Day.	24_{-0}	$24{\cdot}25_{-44{\cdot}4}$	Day.	24_{-0}	$24{\cdot}25_{-44{\cdot}4}$	Day.	24_{-0}	$24{\cdot}25_{-44{\cdot}4}$
1	−233	−224	9	+ 26	+ 24	17	+144	+141
2	−289	−269	10	+ 94	+ 59	18	+179	+139
3	−281	−270	11	+107	+125	19	+204	+119
4	−249	−221	12	+126	+167	20	+190	+ 83
5	−191	−177	13	+130	+191	21	+126	+ 44
6	−125	− 99	14	+101	+180	22	+ 43	− 7
7	− 59	− 57	15	+100	+153	23	− 65	− 73
8	− 23	− 22	16	+117	+148	24	−162	−154



Results of Comparison of Inequalities around 24 Days.

15. *Comparison in Length of Period.*—The comparative magnitudes of the various solar Inequalities, whether apparent or real, are, as we have already explained, estimated by adding together the various yearly results appropriately placed, and then in the final Inequality so obtained by summing up the various departures whether positive or negative. The result for sun-spots is given in the form of a sum consisting of four or five places. Of these we may to save space and time dismiss the two right hand figures, that is to say, divide by 100, while yet retaining a result sufficiently accurate for the purposes of our comparison. We have made this reduction in the following table not merely for sun-spots, but likewise for temperature-ranges. It has to be remembered that for sun-spots and Toronto temperature-ranges the results exhibit the sum of 36 years, while for Kew temperature-ranges they only exhibit the sum of 24 years. The Kew temperature-ranges are esteemed by us to be of subordinate value, partly because they consist of a smaller number of years, and partly because they have only 12 years in common with sun-spots, and they have been here introduced for a reason that will afterwards appear. Meanwhile in our comparison as to length of period we confine ourselves to sun-spots and Toronto temperature-ranges, inasmuch as each of these series embodies 36 years, of which 24 years are common to both.

In the following table we have inclosed in brackets the positions of all sufficiently well-defined maximum Inequalities, whether of sun-spots or Toronto temperature-ranges. We have likewise given a list of the Kew temperature-range Inequalities, but for the reason already mentioned we have not used these for the purpose of comparison, and have not inclosed their maximum points in brackets.

It will be seen that on the whole the positions of maximum apparent Inequality for sun-spots are near those for Toronto temperature-ranges. While this likeness cannot be considered as conclusively proving a connexion, it is nevertheless the sort of similarity which might be expected to exist between phenomena physically connected, but containing so many apparent Inequalities, and these so near together that our series of observations is not sufficiently extensive to enable us to eliminate their influence upon each other, or to allow us to ascertain their true positions.

Table II.—Apparent Inequalities around 24 Days (SS=Sun-spots, TT=Toronto Temperature-range, KT=Kew Temperature-range).*

Period.	SS.	TT.	KT.	Period.	SS.	TT.	KT.	Period.	SS.	TT.	KT.
−48	94	27	61	−14	162	71	40	+20	241	[81]	44
−47	98	29	59	−13	236	66	39	+21	227	75	41
−46	104	48	53	−12	228	58	38	+22	220	64	38
−45	131	[50]	49	−11	251	51	36	+23	151	48	27
−44	162	41	45	−10	248	48	34	+24	146	43	30
−43	214	38	45	− 9	256	41	32	+25	141	52	35
−42	233	37	38	− 8	[257]	34	29	+26	[155]	64	40
−41	258	38	31	− 7	233	29	24	+27	150	[68]	49
−40	[288]	45	26	− 6	232	40	20	+28	143	67	57
−39	255	45	25	− 5	225	52	17	+29	153	61	67
−38	216	[46]	28	− 4	117	[65]	19	+30	176	59	73
−37	175	42	40	− 3	165	64	20	+31	223	55	77
−36	111	42	42	− 2	179	45	24	+32	277	44	72
−35	93	48	43	− 1	267	35	38	+33	321	[48]	75
−34	107	47	51	0	264	37	41	+34	[332]	46	77
−33	149	[50]	50	+ 1	272	42	41	+35	332	43	67
−32	192	46	51	+ 2	[303]	42	55	+36	322	39	67
−31	221	33	48	+ 3	259	[49]	49	+37	294	45	65
−30	[236]	18	46	+ 4	225	43	39	+38	243	56	52
−29	233	24	45	+ 5	208	48	35	+39	197	65	45
−28	221	26	43	+ 6	203	49	30	+40	175	[68]	40
−27	189	29	42	+ 7	172	57	31	+41	239	47	35
−26	179	30	43	+ 8	169	[66]	44	+42	286	30	33
−25	176	43	46	+ 9	189	63	48	+43	332	22	30
−24	163	56	52	+10	202	55	54	+44	[365]	27	30
−23	165	63	55	+11	204	45	55	+45	362	35	26
−22	115	65	46	+12	[241]	32	57	+46	345	42	24
−21	114	66	46	+13	214	19	59	+47	283	47	33
−20	103	69	46	+14	178	23	64	+48	269	47	38
−19	160	70	43	+15	174	31	62				
−18	[179]	67	45	+16	209	42	58				
−17	174	67	46	+17	241	61	61				
−16	104	74	38	+18	276	71	54				
−15	131	[76]	39	+19	[292]	78	48				

16. *Constancy of Type in the various Inequalities.*—It will be seen from Table III that as a rule the sun-spot Inequalities contain one maximum and one minimum in 24 days, and that as a rule the Kew temperature-range Inequalities contain likewise one maximum and one minimum,† while, however, the Toronto temperature-range In-

* The numbers in this table represent the aggregate Inequalities for 36 years in the sun-spot and Toronto temperature-range, and for 24 years in the Kew temperature-range, after dividing by 100. Thus the SS Inequality for −48 is in reality 9386, while the TT and KT are 2744 and 6060.

† At any rate the Kew Inequalities much more nearly represent a single than a double oscillation.

equalities contain distinctly two maxima and two minima. There are doubtless solitary exceptions where the two former Inequalities exhibit two oscillations, and the latter an approach to a single oscillation only, but these are exceptions, and may we think fairly be supposed to be caused by the presence of some irregularity which we have not been able to eliminate. We consider this constancy of type to be favourable to the view of a physical connexion between these phenomena. The argument may, perhaps, be put in this way. We are not entitled to expect in all cases terrestrial periods of one maximum and one minimum to correspond with solar periods of one maximum and one minimum, inasmuch as in another instance, namely, that of atmospheric pressure, we have two maxima and two minima more or less distinct for one solar day. But we are entitled to expect, on the supposition that there is a physical connexion between the phenomena we are investigating, that they should exhibit a constancy of type. This constancy is, therefore, a thing which we might expect on the supposition of a connexion, but a thing unlikely to occur on the supposition that there is no true connexion between these various Inequalities.

17. *Comparison in Phase.*—Not only might we expect on the hypothesis of a physical connexion between them that there should be a constancy of type in these various phenomena, but we are furthermore entitled to look for a definite relation in phase between the corresponding celestial and terrestrial Inequalities.

A comparison in phase seems calculated to afford better evidence of a connexion than a comparison in length of period. Referring for example to Table II, we find a well-marked maximum Inequality which occurs at +19 for sun-spots, and +20 for Toronto temperature-ranges. Had there been no other maximum than this, and had the sums rapidly fallen off on either side of this point, we should naturally have imagined that this nearness was a strong proof of a physical connexion, but there are evidently several apparent Inequalities, and these are somewhat close together.

Thus while there is a certain kind of evidence in the fact that for each sun-spot maximum Inequality we have a Toronto temperature-range Inequality not far distant, yet this evidence is rather derived from the circumstance that for each solar there is a terrestrial maximum than from the circumstance that these occur close together; for if there be a sufficient number of such couplets they must occur close together.

It thus appears that viewed in this way the evidence of a connexion derived from nearness of period becomes the less striking the greater the number of periods. The truth is that if there were only a single Inequality in each record a comparatively short series would enable us to detect a connexion, but if there are many Inequalities close

together, and if, moreover, not only the years of the series but also the type of the Inequality be different in the two records, we should require a much longer series before we could so disentangle the influence of Inequalities upon each other as to point by this means unmistakably to a physical connexion.

The case is different, however, in a comparison of phase. If we are able to show a nearly definite relation in phase between each couplet of these associated phenomena, then the greater the number of such couplets the greater will be the evidence. Were there only a single Inequality we could have no evidence whatever of this kind, but if there be a good number of Inequalities in each couplet of which there is a constant relation in phase between the two members, the evidence of a connexion becomes very strong, because the likelihood that such a relation should constantly repeat itself without a physical connexion is extremely small. Again, as a matter of fact, it is much more easy to alter slightly the position of a maximum than it is sensibly to affect the phase of the Inequality. We have already mentioned in Art. 10 that we have only deemed it necessary to make separate and complete settings for every third of the small stages herein employed. Now suppose that by such treatment we have a maximum at some point, say, for instance, −36. By employing the more laborious method of a complete setting for each small stage we might in some cases shift the position of such a maximum (if only slightly in excess of its neighbours) one or even two places, say to −37 or −38, but we should not by this method of treatment materially alter the phase. In fine, the comparison by periods is subject to greater uncertainty than the comparison by phases, inasmuch as while the maximum has been displaced by a slight difference of treatment, through two divisions, the phases for −36, −37, −38 by the old method are *practically* the same as those by the new. We have already (Art. 9) sufficiently indicated the method of dealing with an Inequality of one period so as to bring it to represent in phase one of a slightly different period. In the above instance, therefore, we may imagine the true period of the Inequality to be −36, or we may with equal propriety imagine it to be −38; in either case we shall have the same phase for our Inequality, provided that in the latter we reduce −38 so as to represent in phase the period −36, or, in fine, bring both to one common period.

It thus becomes manifest what we ought to do. We ought to reduce in phase the two members of a couplet to the same time-scale and then make our comparison. It is possible that if the series of years had been precisely the same for the celestial and the terrestrial members of the couplet, and if both had presented only one oscillation in 24 days, we might have found a constancy of phase between

couplets of similar period even if these might be remote from a position of maximum Inequality. But as it is we must confine ourselves to positions near some maximum in making our comparisons, and inasmuch as the Toronto temperature-range Inequalities presenting two fluctuations in 24 days are virtually short period Inequalities, we have preferred to take the positions of maximum Inequality from them rather than from the sun-spot records.

Thus, for instance, referring once more to Table II, we find a Toronto maximum at −33, and a sun-spot maximum at −30. We have chosen −33 as our period, and have altered the solar maximum at −30 in phase so as to make it indicate −33, and so on with other couplets.

Furthermore, in each case we take the mean of three series. Thus in the instance now recorded we have taken the mean of sun-spots at −29, −30, and −31 all reduced to −33, and have compared with this the mean of Toronto ranges at −34, −33, −32 all reduced to −33 likewise, and so on for other couplets. Finally, in Table III, each series has been reduced so as to represent the sum of 12 years Inequalities, and the sun-spot series have been equalised in the same way as the terrestrial observations, because in instituting any comparison with respect to magnitude it is desirable that the same treatment should have been applied to both.

In a previous part of our paper (Art. 15) we spoke of a subsidiary service which was to be rendered by the Kew temperature-ranges. We propose to regard these as terrestrial pointers or indicators by the aid of which we may distinguish the one Toronto maximum from the other. It will be found that one of the Toronto maxima occurs in date about 8 or 9 days before the single maximum of the corresponding Kew Inequality. This Toronto maximum, so indicated by a strictly terrestrial method, we have fixed upon as our starting point, and at this point all the Toronto Inequalities in Table III are made to begin. The Toronto Inequalities thus placed are then compared with sun-spot and Kew temperature-range Inequalities of similar period, so arranged that the three members of the same triplet always start on the same day; in other words, the numbers on the same horizontal line and belonging to the various members of the same triplet denote simultaneous phenomena.

In order to make this comparison it is necessary that the Inequalities compared together should be consistent with their prevalent type; for instance, the Kew temperature-range Inequality must present a single maximum and minimum sufficiently well at least to admit of serving its office as a pointer. In like manner the Toronto temperature-range must be a double, and the sun-spot a single Inequality. We must, therefore, examine the couplets of Table II in order to see

which of these are fitted to be subjects of comparison. This we now proceed to do.*

SS_{-40}, TT_{-38}. All sufficiently good. Use for all −40, −39, −38, reduced to −39.

SS_{-30}, TT_{-33}. All sufficiently good. Use SS−31, −30, −29; TT−34, −33, −32; KT−34, −33, −32, all reduced to −33.

SS_{-18}, TT_{-15}. *Toronto* too nearly single; all the others good.

SS_{-8}, TT_{-4}. *Kew* Inequality very small and irregular; the others sufficiently good.

SS_{+2}, TT_{+3}. *Kew* Inequality presents two maxima and minima; others good.

SS_{+12}, TT_{+8}. *Toronto* Inequality too nearly single. Sun-spots present also traces of two maxima; Kew temperature good.

SS_{+19}, KT_{+20}. Use for all +17, +18, +19; reduced to +18.

SS_{+26}, TT_{+27}. *Sun-spots* two maxima; others sufficiently good.

SS_{+34}, TT_{+33}. Use for all +32, +33, +34, reduced to +33.

SS_{44}, TT_{+40}. Use SS+43, +44, +45; TT+39, +40, +41; KT+38, +39, +40, all reduced to +40.

It thus appears that out of ten instances sun-spot Inequalities

* It will be desirable to exhibit the rejected inequalities. They are as follows, being the aggregates for 36 years in SS and TT and for 24 years in KT.

Toronto temperature.		Sun-spots.		Kew temperature.	
− 15	+ 8	+ 12	+ 26	− 4	+ 3
+405	−308	+ 419	− 679	− 39	−260
+518	−351	− 362	− 444	− 22	−308
+605	−290	−1203	+ 84	− 48	−222
+461	−268	−1158	+ 236	− 55	− 62
+ 62	−299	− 245	+ 212	− 68	+110
−208	−264	+ 776	+ 56	−127	+364
−500	−294	+1381	+ 46	−112	+440
−609	−304	+ 840	+ 204	−136	+407
−467	−235	+ 230	− 193	−126	+267
−443	−282	+1035	+ 547	− 76	− 12
−346	−216	+1585	+ 298	− 71	−157
−350	− 59	+1319	− 416	+ 72	−249
−397	− 36	+1986	−1403	+184	−311
−334	+ 69	+ 452	− 226	+203	−277
−158	+119	−2163	+ 964	+203	−200
+ 88	+ 43	−1652	+ 489	+ 89	− 89
+260	+158	−1878	+1150	− 12	+ 71
+367	+325	−1846	+1721	− 34	+184
+327	+478	−1098	+1185	+ 6	+233
+233	+687	− 453	+ 546	+ 53	+195
+135	+713	+ 340	−1012	+ 59	+127
+103	+501	+ 612	− 757	+ 56	+ 43
+ 72	+208	+ 333	−1691	+ 21	− 93
+176	− 95	+ 750	− 917	− 20	−201

Table III.—Comparison of Celestial and Terrestrial Inequalities around 24 Days.*

Symbol.			Date of first number (in January, 1832).
24_{-39}	.... 23·7862	....	January 3 or January 27.
24_{-33}	.... 23·8193	....	" 7 " 31
24_{+18}	.... 24·0985	....	" 23
24_{+33}	.... 24·1807	....	" 19
24_{+40}	.... 24·2190	....	" 22

Toronto temperature.						Sun-spots.						Kew temperatures.					
− 39	− 33	+ 18	+ 33	+ 40	Mean.	− 39	− 33	+ 18	+ 33	+ 40	Mean.	− 39	− 33	+ 18	+ 33	+ 40	Mean.
+ 64	+ 56	+202	+ 47	+180	+110	−436	−360	−260	−372	−639	−413	+ 15	−160	−151	+ 47	+ 14	− 47
+ 41	+ 54	+182	+ 40	+156	+ 95	−303	−234	− 94	−340	−549	−304	+ 37	−102	− 56	+ 44	+ 45	− 6
− 3	+ 59	+132	+ 21	+ 73	+ 56	−156	−111	+ 66	−280	−428	−182	+ 46	− 30	+ 20	+ 60	+ 57	+ 30
− 46	+ 46	+ 85	− 4	− 44	+ 7	− 9	+ 13	+206	−179	−289	− 52	+ 36	+ 30	+ 70	+118	+ 55	+ 62
− 90	− 12	+ 48	− 31	−145	− 46	+125	+137	+314	− 28	−139	+ 82	+ 44	+ 55	+ 96	+196	+ 65	+ 91
−108	− 49	+ 4	− 56	−191	− 80	+233	+249	+381	+162	+ 20	+209	+ 64	+ 73	+100	+294	+ 89	+124
−111	− 69	− 41	− 55	−186	− 93	+331	+325	+413	+350	+198	+323	+ 96	+ 51	+128	+367	+139	+156
−120	− 39	− 79	− 48	−137	− 85	+394	+349	+427	+505	+365	+408	+118	+ 30	+156	+341	+185	+166
−106	+ 18	−120	− 11	− 76	− 59	+422	+339	+431	+606	+514	+462	+ 89	+ 18	+191	+239	+191	+145
− 92	+ 68	−128	+ 44	− 16	− 25	+412	+328	+438	+637	+614	+486	+ 18	− 7	+202	+ 95	+152	+ 92
− 56	+108	−115	+ 93	+ 29	+ 12	+364	+330	+436	+615	+643	+478	− 46	+ 9	+161	− 17	+ 66	+ 34
+ 6	+102	− 96	+133	+ 60	+ 41	+326	+350	+402	+558	+623	+452	− 96	+ 34	+121	− 65	− 40	− 9
+ 56	+ 72	− 62	+136	+ 68	+ 54	+306	+354	+324	+464	+574	+404	− 77	+ 55	+ 61	− 84	−125	− 34
+ 94	+ 34	− 58	+104	+ 51	+ 45	+292	+330	+196	+344	+515	+335	− 19	+125	+ 34	−107	−199	− 33
+107	− 30	− 65	+ 45	+ 20	+ 15	+273	+259	+ 45	+182	+450	+242	+ 3	+175	+ 20	−170	−219	− 38
+ 83	−101	− 79	− 31	− 31	− 32	+203	+148	− 98	− 11	+364	+121	+ 15	+199	− 5	−243	−198	− 46
+ 70	−149	−103	− 95	− 55	− 66	+ 87	+ 21	−225	−199	+235	− 16	− 18	+209	− 19	−305	−170	− 61
+ 59	−163	− 95	−132	− 58	− 78	− 66	− 99	−330	−359	+ 61	−159	− 71	+132	− 60	−314	− 93	− 81
+ 27	−129	− 75	−132	− 35	− 69	−237	−228	−425	−447	−148	−297	− 77	+ 18	−102	−270	− 21	− 90
+ 20	− 49	− 36	− 95	+ 3	− 31	−389	−351	−498	−475	−362	−415	− 76	− 85	−143	−192	+ 7	− 98
+ 4	+ 12	+ 37	− 46	+ 28	+ 7	−504	−461	−545	−470	−533	−503	− 53	−179	−178	− 92	+ 25	− 95
+ 9	+ 58	+100	− 1	+ 57	+ 45	−563	−536	−554	−444	−648	−550	− 31	−221	−219	− 11	+ 7	− 95
+ 36	+ 62	+165	+ 29	+ 99	+ 78	−574	−539	−499	−421	−703	−547	− 21	−222	−227	+ 27	− 21	− 93
+ 56	+ 41	+205	+ 45	+150	+ 99	−526	−472	−401	−398	−690	−497	+ 4	−207	−200	+ 42	− 11	− 74

* In this table all the Inequalities are aggregates for 12 years, and have all been equalised in the same manner, see Art. 12. The Toronto Inequality +18 is somewhat irregular, but it has been retained partly to exhibit an instance of comparison in which one of the members departs somewhat from the usual type.

depart from their type in two, Toronto Inequalities in two, and Kew temperature Inequalities in two cases, leaving five unexceptionable triplets for comparison. These are given in Table III (p. 307).

In the preceding table the settings have been arranged by strictly terrestrial considerations. If, therefore, there is no connexion between terrestrial and solar Inequalities, the sun-spot maxima should be distributed impartially up and down the table without any other than chance grouping together. Their behaviour is, however, very different from this, these maxima being comparatively closely grouped together about a position nine days later than that of the Toronto temperature-range maxima which begin the table.

The results of this table are exhibited in Diagram I which accompanies this paper.

Table IV.—Exhibiting Sun-spot Inequality 26_{+43}, split up into Three Series of Twelve Years each.

Unequalised.				Equalised.			
1832–43.	1844–55.	1856–67.	Whole.	1832–43.	1844–55.	1856–67.	Whole.
− 535	− 1037	− 775	− 2347	− 495	− 1050	− 589	− 2134
− 1131	− 1417	− 782	− 3330	− 903	− 1193	− 761	− 2857
− 1031	− 1345	− 994	− 3370	− 1157	− 1154	− 915	− 3226
− 1618	− 832	− 882	− 3332	− 1333	− 1043	− 992	− 3368
− 1283	− 819	− 1146	− 3248	− 1346	− 934	− 1119	− 3399
− 1383	− 1151	− 1224	− 3758	− 1275	− 869	− 1135	− 3279
− 1231	− 642	− 1424	− 3297	− 1134	− 814	− 1036	− 2984
− 683	− 617	− 522	− 1822	− 1060	− 801	− 757	− 2618
− 1101	− 1045	− 412	− 2558	− 1040	− 761	− 517	− 2318
− 1445	− 834	− 350	− 2629	− 903	− 573	− 332	− 1808
− 573	− 174	− 252	− 999	− 579	− 240	− 206	− 1025
+ 244	+ 404	− 92	+ 556	− 190	+ 92	− 96	− 194
+ 11	+ 271	+ 198	+ 480	+ 86	+ 322	+ 25	+ 433
+ 128	+ 259	− 126	+ 261	+ 365	+ 490	+ 187	+ 1042
+ 573	+ 933	+ 316	+ 1822	+ 735	+ 747	+ 526	+ 2008
+ 1490	+ 823	+ 1124	+ 3437	+ 1159	+ 1025	+ 985	+ 3169
+ 1653	+ 1490	+ 1597	+ 4740	+ 1415	+ 1330	+ 1407	+ 4152
+ 1381	+ 1653	+ 1791	+ 4825	+ 1478	+ 1477	+ 1585	+ 4540
+ 1459	+ 1613	+ 1723	+ 4795	+ 1435	+ 1463	+ 1463	+ 4361
+ 1442	+ 1308	+ 1132	+ 3882	+ 1313	+ 1281	+ 1130	+ 3724
+ 1242	+ 919	+ 560	+ 2721	+ 1114	+ 1036	+ 754	+ 2904
+ 710	+ 885	+ 413	+ 2008	+ 872	+ 777	+ 472	+ 2121
+ 535	+ 572	+ 290	+ 1397	+ 680	+ 459	+ 261	+ 1400
+ 722	+ 50	+ 178	+ 950	+ 531	+ 60	+ 103	+ 694
+ 408	− 376	− 261	− 229	+ 300	− 364	− 119	− 183
+ 16	− 891	− 80	− 955	− 68	− 763	− 324	− 1155
+12014	+11180	+9322	+31874	+11483	+10559	+8898	+30548
−12014	−11180	−9322	−31874	−11483	−10559	−8898	−30548

Results of Comparison of Inequalities around 26 Days.

18. The solar Inequalities around 26 days are remarkably large and well defined; and, without attempting to prejudge the question

whether they are real or only apparent Inequalities, it may be interesting to exhibit in Table IV (p. 308) the largest solar Inequality with which we have as yet come into contact. The figures given represent the sums of twelve yearly Inequalities, the whole 36 years being split into three series of 12 years each. The amount of repetition will be seen from a comparison of these three series, which are exhibited, first, without any equalisation, and secondly, with the same amount of equalisation which has been applied to the terrestrial Inequalities around 26 days (Art. 12).

19. *Comparison in Length of Period.*—The comparative magnitudes of the various solar Inequalities around 26 days are estimated after the manner already described—that is to say, by adding together the various yearly results appropriately placed, and then in the final Inequality so obtained summing up the various (26) departures, whether positive or negative.

The two right hand figures are dismissed both for celestial and

Table V.—Apparent Inequalities around 26 Days.

(The numbers in this table represent the aggregate Inequalities for 36 years in the sun-spots and Toronto temperature-ranges, and for 24 years in the Kew temperature-ranges, the whole being divided by 100.)

Period.	SS.	TT.	KT.	Period.	SS.	TT.	KT.	Period.	SS.	TT.	KT.
−54	197	63	...	−20	240	59	101	+14	481	69	69
−53	210	77	89	−19	297	56	101	+15	[503]	58	70
−52	298	[94]	81	−18	264	43	98	+16	496	57	69
−51	359	83	76	−17	292	36	94	+17	280	63	69
−50	361	85	71	−16	327	31	91	+18	345	54	70
−49	454	80	65	−15	394	40	85	+19	280	41	70
−48	[456]	53	62	−14	456	54	75	+20	234	42	73
−47	453	60	59	−13	429	50	69	+21	247	47	76
−46	451	59	57	−12	459	68	57	+22	234	48	75
−45	407	56	56	−11	[472]	[80]	44	+23	290	58	74
−44	321	61	54	−10	345	68	50	+24	304	57	75
−43	273	68	63	− 9	320	63	43	+25	[340]	51	72
−42	271	63	64	− 8	266	68	37	+26	322	37	58
−41	259	69	63	− 7	124	67	35	+27	321	47	55
−40	180	48	66	− 6	128	63	39	+28	315	73	49
−39	168	39	69	− 5	159	[68]	50	+29	230	[88]	33
−38	167	60	74	− 4	231	55	50	+30	218	61	30
−37	149	59	82	− 3	284	49	58	+31	179	52	28
−36	168	60	84	− 2	[340]	37	66	+32	140	70	31
−35	205	51	83	− 1	304	44	72	+33	115	68	40
−34	276	48	81	0	319	44	80	+34	168	55	46
−33	296	43	76	+ 1	320	43	86	+35	211	43	51
−32	317	28	70	+ 2	213	49	77	+36	269	44	50
−31	384	58	45	+ 3	218	51	77	+37	320	55	43
−30	382	60	39	+ 4	232	48	76	+38	411	66	35
−29	[410]	[62]	34	+ 5	137	45	78	+39	493	69	41
−28	387	58	45	+ 6	181	44	78	+40	552	62	43
−27	373	50	51	+ 7	212	43	78	+41	619	73	55
−26	350	40	60	+ 8	200	57	74	+42	632	[73]	56
−25	351	44	73	+ 9	238	78	76	+43	[637]	70	59
−24	332	53	80	+10	308	[84]	75	+44	596	47	55
−23	287	51	85	+11	358	70	64	+45	559	42	50
−22	[337]	[72]	103	+12	425	[80]	67	+56	510	...	47
−21	297	71	103	+13	478	74	69	+47	374	...	59

terrestrial Inequalities as in the previous instance. The series of years is the same, and the same use is made of the Kew temperature-ranges as in the Inequalities around 24 days.

In the Table V (p. 309) we have, inclosed in brackets, the positions of all sufficiently well-defined maximum Inequalities, whether of sun-spots or Toronto temperature-ranges. It will be seen, as in the previous case, that the positions of maximum apparent Inequalities for sun-spots are near those for Toronto temperature-ranges; and the same remarks are here applicable which have already been made.

20. *Constancy of Type in the various Inequalities.*—Here, as in the Inequalities around 24 days, the Toronto temperature-ranges present on the whole two maxima and two minima, the sun-spots one well-defined maximum and minimum; and the Kew temperature-ranges one maximum and minimum, not so well defined.

21. *Comparison in Phase.*—For the purpose of making this comparison, we proceed to discuss the various couplets of Table V, taking as before the time-scales in our comparison from the Toronto temperature Inequalities rather than from the sun-spots.*

SS_{-48}, TT_{-52}. *Toronto* temperature-range Inequality not sufficiently near the type; the others good.

* It will be desirable to exhibit the rejected Inequalities. They are as follows, being the aggregate for 36 years in TT, and for 24 years in KT:—

Toronto temperature.			Kew temperature.	
− 52	− 22	+ 29	− 5	+ 29
−103	+189	− 96	+257	+ 35
− 58	+ 29	+151	+404	− 34
+ 55	−157	+331	+222	− 65
+164	−295	+364	− 67	−113
+215	−177	− 59	−415	−122
+152	+ 2	−519	−586	− 28
+ 70	+258	−781	−528	+125
− 26	+334	−302	−327	+206
− 51	+195	+190	+ 4	+ 32
− 88	− 20	+504	+ 99	−179
−136	−311	+257	+ 91	−367
−207	−380	+124	−118	−283
−215	−274	+224	−158	−197
−173	+ 31	+428	−107	− 76
− 95	+466	+230	+134	− 6
− 64	+666	−289	+321	+131
− 41	+782	−739	+339	+222
− 24	+413	−743	+246	+271
+ 22	− 29	−505	+144	+241
+ 88	−571	+ 1	+103	+216
+152	−633	+366	+ 63	+108
+183	−498	+683	− 34	+ 2
+156	−181	+420	− 35	− 88
+ 87	− 72	+148	− 82	− 83
+ 5	+ 81	−210	− 43	+ 7
− 68	+152	−178	+ 73	+ 45

Table VI.—Comparison of Celestial and Terrestrial Inequalities around 26 Days.*

Symbol.	Period in days.	Date of first number in January, 1832.
26_{-29}	25·8279	January 21
26_{-11}	25·9347	„ 15
26_{+10}	26·0593	„ 18
26_{+42}	26·2492	„ 13

Toronto temperature.					Sun-spots.					Kew temperature.				
− 29	− 11	+ 10	+ 42	Mean.	− 29	− 11	+ 10	+ 42	Mean.	− 29	− 11	+ 10	+ 42	Mean.
+147	+ 66	+132	+160	+126	−101	+372	− 862	− 11	−150	+ 9	+ 65	+ 33	+ 80	+ 47
+145	+ 35	+ 98	+185	+116	+ 91	+414	− 789	+ 208	− 19	+ 74	+ 37	+ 76	+ 31	+ 55
+ 94	+ 31	+ 36	+114	+ 69	+286	+441	− 665	+ 427	+122	+125	+ 39	+110	− 29	+ 61
+ 42	+ 33	− 1	+ 0	+ 18	+463	+466	− 499	+ 699	+282	+147	+ 83	+173	− 37	+ 92
− 46	+ 10	− 48	−119	− 51	+599	+512	− 282	+ 981	+452	+170	+135	+291	− 18	+144
− 92	− 54	− 52	−158	− 89	+661	+588	− 28	+1248	+617	+193	+184	+341	+ 59	+194
−126	−103	− 5	−151	− 96	+659	+687	+ 253	+1404	+751	+190	+203	+302	+117	+203
− 99	− 77	+ 92	−134	− 54	+637	+771	+ 537	+1412	+839	+155	+182	+183	+140	+165
− 79	+ 19	+167	−152	− 11	+586	+767	+ 806	+1258	+854	+144	+108	+ 61	+104	+104
− 36	+124	+187	−127	+ 37	+560	+645	+1007	+1013	+806	+177	− 2	− 16	+ 44	+ 51
− 10	+167	+130	− 43	+ 61	+514	+422	+1107	+ 752	+699	+200	− 75	− 36	+ 8	+ 24
+ 59	+159	+ 56	+108	+ 95	+475	+188	+1097	+ 486	+561	+112	−100	− 22	− 25	− 9
+ 61	+132	− 34	+162	+ 80	+383	− 28	+1015	+ 197	+392	− 25	− 58	− 53	− 43	− 45
+ 47	+115	−107	+145	+ 50	+273	−238	+ 868	− 123	+195	−151	− 33	−140	− 95	−105
− 32	+ 68	−139	+ 28	− 19	+116	−469	+ 662	− 455	− 36	−196	− 53	−243	−159	−163
− 41	+ 12	−110	− 29	− 42	− 68	−687	+ 381	− 748	−280	−230	−102	−246	−243	−205
− 41	− 15	− 35	− 75	− 41	−296	−833	+ 85	− 960	−501	−238	−166	−190	−248	−210
− 34	− 1	− 15	− 71	− 30	−504	−887	− 159	−1062	−653	−243	−163	− 93	−188	−172
− 61	− 13	− 68	− 66	− 52	−679	−887	− 313	−1084	−741	−184	−146	− 62	− 61	−113
− 73	−132	−162	− 23	− 98	−778	−855	− 408	−1065	−776	−157	−101	− 17	+ 39	− 59
− 34	−248	−201	+ 8	−119	−827	−779	− 473	−1036	−779	− 99	− 92	+ 6	+114	− 18
− 8	−281	−164	+ 21	−108	−818	−603	− 517	− 996	−733	− 66	− 76	+ 10	+127	− 1
+ 22	−164	− 74	+ 10	− 51	−763	−361	− 562	− 926	−653	− 21	− 44	− 59	+101	− 6
+ 18	− 31	+ 37	+ 33	+ 14	−659	− 81	− 644	− 791	−544	− 18	+ 17	−157	+ 52	− 26
+ 67	+ 65	+117	+ 56	+ 76	−502	+141	− 765	− 555	−420	− 37	+ 68	−168	+ 56	− 20
+110	+ 83	+163	+118	+119	−308	+294	− 852	− 273	−285	− 31	+ 90	− 84	+ 74	+ 12

* In this table all the Inequalities are aggregates for 12 years, and have all been equalised in the same manner (see Art. 12). The Toronto Inequality +10 is somewhat irregular, but it has been retained, partly to exhibit an instance of comparison in which one of the members departs somewhat from the usual type.

SS_{-29}, TT_{-29}. All sufficiently good. Use TT −30, −29, −28. SS −30, −29, −28, and KT −26, −25, −24, all reduced to −29.

$SS_{-22}TT_{-22}$. *Toronto* temperature-range Inequality irregular; the others sufficiently good.

SS_{-11}, TT_{-11}. All sufficiently good. Use TT −12, −11, −10; SS −12, −11, −10; KT −12, −11, −10, all reduced to −11.

SS_{-2}, TT_{-5}. *Kew* Inequality irregular; the others good.

SS_{+15}, TT_{+10}. Sufficiently good. Use TT +9, +10, +12; SS +14, +15, +16; KT +9, +10, +11; all reduced to +10.

SS_{+25}, TT_{+29}. *Toronto* Inequality irregular, also *Kew* Inequality; sun-spots good.

SS_{+43}, TT_{+42}. All sufficiently good. Use TT +41, +42, +43; SS +41, +42, +43; KT +39, +40, +41; all reduced to +42.

It thus appears that, out of eight cases, the sun-spots do not depart from their type in a single instance, while Toronto temperature-range Inequalities are too irregular to admit of comparison in three, and Kew temperature-range Inequalities in two cases, leaving four unexceptionable cases for comparison. These are exhibited in Table VI and in Diagram II in a manner exactly similar to that in which the 24-day Inequalities were exhibited in Table III and in Diagram I. Also from Table VI we may draw conclusions exactly similar to those which we drew from Table III, the two tables exhibiting virtually the same behaviour of the various Inequalities.

Concluding Remarks.

22. We would remark, in the first place, that there are two possible kinds of periodicity with respect to sun-spots, and that it is not necessary to regard the Inequalities around 24 days and those around 26 days as perfectly similar phenomena. To illustrate our meaning, let the reader imagine that some one definite hemisphere of the sun is in the habit of periodically losing its brilliancy and then becoming luminous again, while, however, the other hemisphere remains constant in its lustre. We may imagine that, in the course of time, an inhabitant of the earth would view these phenomena from all possible quarters and in all possible phases, the result of which would be that, on an average, there would be a falling off in light corresponding to the times of extinction, and no falling off corresponding to the times of maximum lustre. We should, therefore, have what might be termed a solar Inequality of *the first kind.*

Next, suppose that the sun by rotation carries round this state of things, and thus brings next the earth the peculiar solar meridian which passes through the central point of the darkness. Sometimes the earth would view this meridian at its blackest and sometimes at its brightest, but, on the whole, it would present an average lustre midway

between brightness and blackness. On the other hand, the meridian on the opposite side of this would pass before the earth always bright. There would thus be a period representing the synodical period of one solar rotation with respect to the earth, and this might be termed a solar Inequality of *the second kind.*

There seems some reason, from the observations of Broun and Hornstein, to think that peculiar districts, if not a peculiar hemisphere, of the sun may be affected by spots in the way we have mentioned, the result of which would be that we should have amongst the Inequalities of sun-spots one representing the synodical period of revolution of the sun with respect to the earth. Professor Stokes has likewise suggested the desirability of seeing whether there may not be such a period. Now it seems to us that the proof in favour of such a period would not be invalidated by the occurrence of several periods around 26 days; inasmuch as observations founded on sun-spots might present the same variety of period, when treated as we have treated them, which they presented when treated in another way by Carrington, who found that the spots in one solar latitude had a different period of rotation from those in another. It seems, therefore, quite possible that the periods around 26 days may denote the synodical period with respect to the earth of certain spot-producing solar centres not far distant from the equatorial regions, where, according to Carrington and others, the rotation is quickest.

The Inequalities around 24 days have, however, too short a period to admit of the same interpretation being put upon them, inasmuch as they correspond more nearly with the time of rotation of the sun as regards a fixed point in space than with his time of rotation as regards the earth; and here we come to a fact which is worthy of being recorded, although we cannot pretend to discuss it exhaustively in our present paper. It is that the Inequalities around 26 days represent with much accuracy what would be the synodic periods with respect to the earth of the Inequalities around 24 days. In other words, the two sets of Inequalities appear to be related to each other, as if the 24-day series represented in time not merely the period of a variable state of things, but likewise one revolution with regard to a fixed line of space; while the second or 26-day series represents one revolution of the same phenomenon with respect to the earth regarded as the point of view. This would seem to indicate, if confirmed by further inquiry, that the Inequalities around 24 days have reference to something without the sun,* inasmuch as we may imagine the maxima and the minima of the 24-day Inequalities to take place when certain districts of the sun come opposite certain fixed positions of space.

* This peculiar correspondence is perhaps best indicated by the Toronto Temperature-range observations which we think may offer the most trustworthy means of

But while it is not certain that the sun-spot Inequalities around 26 days are phenomena precisely similar to those around 24 days, it would appear that the two sets of solar Inequalities are related to corresponding terrestrial Inequalities as nearly as possible in the same way, any small difference between Tables III and VI being as likely due to insufficient observations as to any physical cause. We may now bring together the results we have obtained as follows:—

α. *Sun-spot Inequalities around 24 and 26 days, whether apparent or real, seem to correspond closely in period with terrestrial Inequalities as exhibited by the daily temperature-range at Toronto and at Kew.*

β. *While the sun-spot and the Kew temperature-range Inequalities present evidence of a single oscillation, the corresponding Toronto temperature-range Inequalities present evidence of a double oscillation.*

γ. *Setting the Inequalities as we have done, the sun-spot maximum occurs about eight or nine days after one of the Toronto maxima, and the Kew maximum about seven days after the same Toronto maximum.*

δ. *The proportional oscillation exhibited by the temperature-range Inequalities is much less than that exhibited by the corresponding solar Inequalities.*

It must be borne in mind that the truth of a connexion between celestial and terrestrial phenomena can only be decided by cumulative evidence of various kinds.

What we claim to have here done is to have given reasons for supposing that there is a correspondence in time-scale and a definite relation in type and phase between sun-spot and temperature-range Inequalities,

estimating periods with exactness. If we slightly equalise the Inequalities of Tables II and V by taking means of three, we obtain the following result:—

Chief prominences around 24 days.	Synodical period of the Inequalities of last column with respect to the earth.	Actual position of chief Inequalities around 26 days.
24_{+3}	26_{-50}	26_{-51}
24_{+8}	26_{-44}	26_{-42}
24_{+20}	26_{-31}	26_{-30}
24_{+27}	26_{-24}	26_{-21}
24_{+39}	26_{-11}	26_{-11}
*24_{+62}	26_{+13}	26_{+11}

In the above table a difference of *one* in the significant figures denotes a difference of time-scale $=\frac{\cdot 0712}{12}=\cdot 0059$ of a day, an extremely small amount.

* This last is beyond the limits of the table.

and this so close and consistent, that the result cannot readily be attributed to mere chance. But if not fortuitous, it tends to support the theory which maintains that there is a sensible relation between the state of the sun's surface as evinced by spots and the meteorology of the earth.

Diagram 1.—Celestial and Terrestrial Inequalities around 24 Days.

Toronto temperature. Sun-spots. Kew temperature.

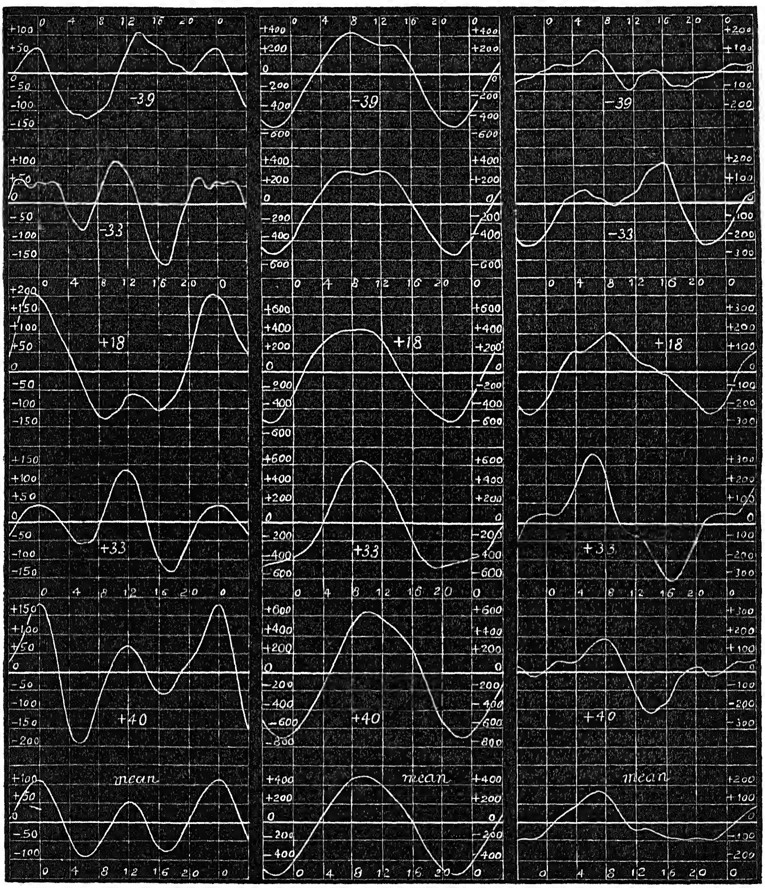
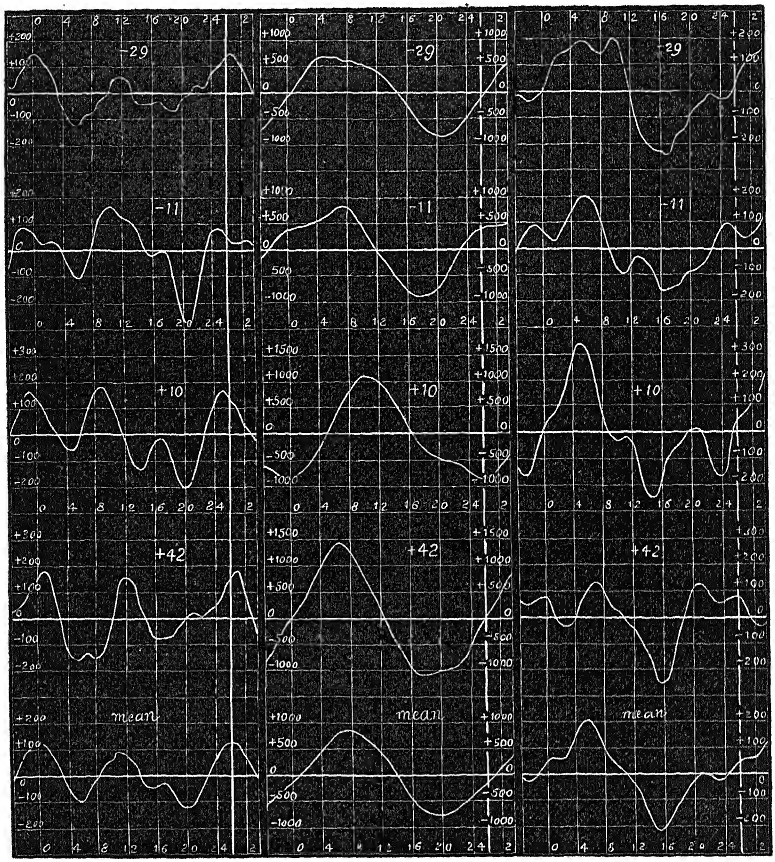


Diagram II.—Celestial and Terrestrial Inequalities around 26 Days.

Toronto temperature. Sun-spots. Kew Temperature.



THE BAKERIAN LECTURE. "Experiments on the Discharge of Electricity through Gases. Sketch of a Theory."* By ARTHUR SCHUSTER, Ph.D., F.R.S. Received May 7. Read June 19, 1884.

[PLATE 4.]

In the following pages I give an account of several new phenomena, some of which have a direct bearing on the theory of the discharge of electricity through gases. Although I hope that the experiments may be judged independently of any theory, their description may be rendered a little more clear and interesting if I begin with a short explanation of the theoretical views to which they seem to me to point.

These theoretical conclusions will be looked at favourably, I think, by those who have been trained up with the ideas of Faraday and Maxwell; for they must have always considered it likely that the conduction of electricity through gases is more probably due to something resembling the electrolytic conduction in liquids than to the direct passage of electricity from one molecule to another. My experiments with mercury vapour argue very strongly in favour of this view.

Fundamental Points of the Theory.

I know, from private conversations, that many of those well able to judge consider some electrolytic action in a gas as probable whenever an electric discharge takes place through it. But owing, no doubt, to the want of experimental evidence, very little has been published bearing on this point. Mr. J. J. Thomson† has recently supported this view. His ideas are of utility for the explanation of some phenomena attending the spark discharges, but they furnish no clue towards the explanation of the asymmetry of the glow discharge, and of some appearances which by many are considered as characteristic and fundamental in the passage of electricity through gases. No consistent attempt towards a systematic theoretical discussion of these facts has yet been made.

I am afraid that I can only offer a very crude beginning of such a discussion, but I have sufficient confidence in the correctness of its fundamental views to submit them to the judgment of the scientific

* I have not in this preliminary paper been able to do full justice to those to whose labours I think it is chiefly due that this subject has become ripe for theoretical deduction. I may mention here the names of De La Rue and Müller, Crookes, Goldstein, Gustav Wiedemann and Rühlmann, and Eilhard Wiedemann; but the two authors whom I have most often found it necessary to consult are undoubtedly Faraday and Hittorf.

† "Phil. Mag.," June, 1883.

public. I shall avoid as much as possible all suppositions and hypotheses which cannot be put to the test of experiment; but it seems necessary to start with some assumption, in order to avoid too great a vagueness in the subsequent explanations. The assumption which I shall make is this: In a gas, the passage of electricity from one molecule to another is always accompanied by an interchange of the atoms composing the molecule. I have been much struck with the results of some experiments recently made by Mr. L. J. Blake in Professor von Helmholtz's laboratory; investigating the vapours rising from the surfaces of warm liquids which were kept charged to a high potential, he found that these vapours were entirely unelectrified. Different liquids were tried—such as pure water, water containing salts in solution, and mercury. Now, if it is found, that the vapour actually rising from an electrified liquid is not electrified, can we imagine that a molecule striking an electrified surface in its rapid motion should carry away with it any part of the electricity, or that one molecule should be able to communicate electricity to another in an encounter? Assuming that we cannot, we are led at once to the supposition that the discharge in gases is accompanied by a breaking up of the molecules. But this does not seem to me to be quite sufficient to account for all phenomena, and especially not for those involving stratifications. There is reason to think that the formation of unstable compounds, such as ozone, plays an important part in the glow discharge. It is very likely essential to the production of stratifications.

The test of any theory must be found in the numerical results it is capable of giving. Hitherto, however, the qualitative phenomena have not, in my opinion, been sufficiently separated from the great number of disturbing effects to allow us to give a decisive value to quantitative measurements. Though I shall not discuss the few measurements which I have been able to make, I shall point out where numerical results can be most easily obtained by the theory and tested by experiment. I do not pretend to explain all the phenomena of the discharge, but I shall attempt to show that there is no distinct contradiction between the theory, in favour of which I argue, and the facts; that is more than can be said of any other theory that has been proposed. I shall try to prove in the present paper that the molecules are, in all probability, broken up at the negative pole.

Experiments with Mercury Vapour.

According to the kinetic theory of gases, the molecule of mercury vapour consists of a single atom, which is incapable of vibration. Mercury has a very brilliant spectrum, which proves that the theory is incomplete in some important point. It is well known, on the other hand, that the theoretical conclusion receives support from the

fact that the vapour-density of mercury vapour is anomalous. If, as is generally supposed, the molecule of the simple gases contains two atoms, that of mercury can only contain one. At any rate, we are justified in asserting that the molecule of mercury vapour has a simpler composition than that of most gases. If, then, an essential part of the glow discharge is due to the breaking up of the molecules, we might expect mercury vapour to present other and much simpler phenomena than the gases with which we are generally accustomed to work. *This, indeed, is the case; for I find that if the mercury vapour is sufficiently free from air, the discharge through it, shows no negative glow, no dark spaces, and no stratifications.* In view of the importance of the fact, I have paid a good deal of attention to this point, and, in spite of some experimental difficulties, I have considerable confidence that the statement which I have made is true. The one fact which up to recently has made me speak with caution consists in the altered conditions under which the experiments with mercury have to be carried on, but I think that I have now a complete answer to the objection which might be raised on that ground.

One difficulty consists in the choice of electrodes. Aluminium is attacked by mercury vapour, and experiments made with aluminium might therefore be objected to as not conclusive. Platinum is the only metal which can be conveniently used, but platinum blackens the tube very rapidly and prevents the examination of the parts surrounding the negative electrode. I have found it convenient to use platinum electrodes surrounded by a small glass tube open at its end. The discharge can then pass freely from the open end, and the glass tube prevents the deposit of platinum in the outer tube. But the platinum may even protrude to a small extent, as the deposit, unless it is thick, seems to combine and alloy easily with the mercury in the tube. If the glass tube surrounding the metal electrode does not fit too closely the discharge will partly pass through the interstice and heat up the glass, which then becomes a sufficiently good conductor to allow the discharge to pass through it, which is an advantage, as the electrodes then have a larger area of active surface. My tubes had two such electrodes, and contained also a quantity of mercury, varying perhaps from 1 to 4 cub. centims. They were attached to a Sprengel pump. Both sulphuric acid and phosphoric anhydride were used for drying purposes. After all the air was as much as possible exhausted, the mercury was heated up to its boiling point. When this had repeatedly been done and the tube kept at the pump for at least a day, the discharge from my coil, which gives a good 4-inch spark in air, would not pass, and there was not a trace of fluorescence. Aluminium electrodes gave more trouble than platinum ones, for they seemed to contain much more hydrogen.

It is needless to describe all the tubes used and the experiments

made. I have varied the conditions of the experiments in many ways, but always with the same result. When the air was sufficiently expelled, the spark does not break through the tube at the ordinary temperature; when the mercury is heated the discharge passes, but always in a perfectly continuous stream, joining the positive and negative electrode. It is difficult to prevent small bubbles of mercury from condensing on the surface of the glass, and the absence of dark space and glow might be explained by the irregularities thus introduced, but I think the following observations conclusive in this respect.

The mercury in the tube would always at first contain small bubbles of air between it and the glass, and these could be purposely left, say, on one side, by volatilising the mercury always on the other. One tube could in this way be prepared which, with a small but visible bubble on one side kept down by the mercury, did not allow the discharge to pass. If the mercury was heated without disturbing the bubble, the current passed, illuminating the tube with a perfectly continuous stream of light. As soon, however, as the bubble was driven into the tube, a perfectly distinct and clear dark space became visible at once.

The following observation was also repeatedly made. As the electrodes generally contain a large quantity of air dissolved in them, I have been accustomed to send through my various tubes a very strong discharge, using a Wilde dynamo machine as a primary current. The currents thus obtained I have found very useful in driving out the air from the electrodes. While the mercury was kept hot a strong current was thus sent through the tubes. Instantaneously a dark space appeared surrounding the negative electrode; the appearance presented was that of a discharge through mercury superposed on one through air, and whether these discharges passed through the tube simultaneously or one after the other, the result proved that the dark space and glow can and generally do appear under the exact conditions under which they are absent in mercury vapour. The strong current of the dynamo machine has generally been fatal to the life of the tube, and I am not sure that I have ever been able to examine the discharge from electrodes really free of air. To that small remnant of air it is due, perhaps, that the discharge, though continuous, was seldom quite symmetrical with respect to the two poles. There generally seemed to be a tendency to surround the negative, and to set out only from the point of the positive electrode. But the appearance was much disturbed by globules of condensed mercury on the electrodes; sometimes both electrodes were equally surrounded by the discharge, and sometimes the latter would pass only from the points.

[There are, however, other causes at work which tend to produce

an asymmetry. I have noticed a very curious tendency of the discharge at the negative pole, to start rather from the glass out of which the electrode protrudes, than from the metal electrode. The glass is heated up in that case and a bright yellow, sodium spot is seen at the point from which the current in the vapour sets out. When the discharge sets out from the metal an amalgam of mercury and platinum, or mercury and aluminium, is formed. The subject requires further investigation.—July, 1884.]

It is more difficult to prevent stratifications than to prevent the dark space and glow. A mixture of air and mercury shows beautiful stratifications. When the air is expelled as much as possible, I never saw stratifications while the tubes were attached to the pump, but in order to study the phenomena a little better I sealed off some of the tubes and heated them up either in a steam or paraffin bath, so as to give them the same temperature throughout. It is then that the stratifications are very apt to occur at a temperature of 100° C., when the tension of mercury vapour would, according to Regnault, be three-quarters of a millimetre. The extreme difficulty, however, of excluding all traces of air easily explains this, while the irregularity of the phenomenon, and its absence in the experiments which were carried on with the greatest care, seem to me to be conclusive in proving that pure mercury vapour never shows any stratifications, for the experiments were continued until the tension of mercury vapour was 20 millims.

My assistant, Mr. Arthur Stanton, has taken some photographs of the spark in mercury vapour at different pressures.

Figs. (1*a*) and (1*b*), Plate 4, represent the spark in mercury at a uniform temperature of 50°. The tension of mercury vapour at that temperature according to Regnault is 0·1 millim. Figs. 2*a* and 2*b* represent the discharge at a temperature of 100° and a tension of 0·7, while in figs. 3*a* and 3*b* the temperature was 150° and the tension 4·3 millim. In the figures marked *a* the pole on the left hand side was negative, while in the figures marked *b* the negative pole was to the right. The electrodes were platinum wires surrounded by glass tubes open at the end; and in figs. 2 and 3 the tendency of the discharge at the negative pole to start as far back as possible from the glass, rather than from the metal, is well seen.

I have reason to think that the vapour of sodium resembles that of mercury, and does not show any dark intervals. From the fact that at the temperature of the spark sodium shows its line spectrum instead of the bands, we may conclude that we have also to deal in that case with a monatomic molecule.

Evidence of Dissociation during the Discharge.

There is very strong spectroscopic evidence that gases through

which an electric current is passing are always in a state of rapid dissociation. Thus I do not know of a single case in which the gas during the glow discharge does not show two, or generally even three, distinct spectra in the same or in different parts of the tube. This dissociation seems especially strong in the negative glow. The characteristic spectrum which gases, as a rule, show in the neighbourhood of the negative electrode bears evidence of belonging to a complex molecule, but at the same time a high temperature line spectrum is always seen to overlap the bands. I have shown some time ago that the spectrum of the negative pole cannot be explained by mere peculiarities of temperature, and, further, that it must be due to the formation of a distinct molecule, as the spectrum keeps its original place even a short time after the current has been reversed.

As the subject is one that is not much studied, I may be permitted to give here a short account of the spectral phenomena seen in tubes filled with different gases.

Nitrogen.—This gas shows in the positive half a well-known complicated band spectrum. The glow shows peculiar bands, but the strongest nitrogen *lines* are also always visible, and it is generally only there that the lines are seen. The capillary part will only show lines when its bore is very small, as, for instance, when a piece of thermometer tubing is used. As the bands of the positive half are also seen in the glow, this region shows the superposition of three distinct spectra. There is at present no longer any doubt that this means the superposition of three distinct sets of molecules.

Oxygen shows phenomena exactly corresponding to those in nitrogen. The positive part as regards character shows what I have called the complex line spectrum, which stands in a remarkable way between a line and a band spectrum. The negative glow shows characteristic bands, and at the same time both the spectrum of the positive pole and traces of the high temperature line spectrum of oxygen. Here, again, we have three distinct sorts of molecules in the glow.

The phenomena in *hydrogen* are not as yet so well ascertained. The positive half and also the glow show generally a band spectrum which now is generally ascribed to hydrogen; but, at the same time, the lines are very strong. I do not know whether a peculiar spectrum has ever been noticed or looked for at the negative pole, but, owing to the complexity of the band spectrum, it would be rather difficult to discover. No gas shows greater difficulties of purification, and therefore the spectroscopic appearances are much more doubtful in this than in the other gases.

Carbon compounds show their characteristic spectra whenever they are not very rapidly decomposed; but at the same time they always show at least in the negative glow the true carbon spectrum. I have

already on a former occasion drawn attention to the very suggestive fact that the compounds in which the molecules are made up of two or more carbon atoms (cyanogen, hydrocarbons) give the carbon bands, while those containing only one atom (carbonic oxide) show the true line spectrum of carbon. Ångström and Thalén observe that the violet line of carbon is generally visible in tubes filled with carbonic oxide, but I have only observed this line in the negative glow, where it undoubtedly is visible even when no trace of it can be seen in any other part of the tube. I have also observed that tubes filled with carbonic oxide show a band peculiar to the negative pole. Though this observation is nearly ten years old, I have hitherto left it unpublished together with other matter bearing on the same point, as I had hoped to find time to complete my investigations and to write a general description of the spectra of carbon compounds.

I do not think that any of the other gases have been sufficiently well investigated to be noticed here; but some observations which I have made at different times on the spectrum of chlorine confirm the general conclusion which I have drawn.

Messrs. De La Rue and Müller have noticed in some of their experiments a sudden expansion of the gas when the discharge was sent through it. They have proved that this expansion is not due to increased temperature, and a breaking up of the molecules into atoms seems to me to be its next natural explanation.

While we thus have ample evidence of dissociation whenever an electric current passes through a gas, we also find that whatever increases independently the dissociation improves the conducting power of the gas. Thus we know that a flame is a good conductor, and Hittorf has, in a series of very interesting experiments, shown that if we heat up the electrodes to a white heat, an electromotive force of a few volts will send a current through the gas.

The fact also discovered by Hittorf, that if a discharge is set up a small electromotive force is sufficient to pass a current across, is also easily explained by our theory, as the original discharge throws the molecules into that state of dissociation which favours the passage of the current.

On the Influence of a Positive Electrode on the Negative Glow.

I now pass to the description of a series of experiments which seem to me to be only capable of explanation by the views brought forward in this paper, and I should like therefore to consider them as crucial experiments which have to be explained by any true theory of the electric discharge.

It was up to recently believed that the form and extent of the negative glow was determined only by the shape of the negative electrode, but was independent of the shape of the vessel or the

neighbourhood of another electrode. Goldstein has, however, described some effects of negative electrodes on each other, and E. Wiedemann has recently made an experiment in which some remarkable effects were seen on the approach of the positive electrode to the kathode. Professor Wiedemann's experiment belongs to the same class as those presently to be described, but differs from mine in so far as it refers to very great exhaustion at which the glow is no longer visible, while I have generally worked at much higher pressures and especially studied the effect on the glow. The peculiarity of the arrangement which I have used consists in the large size of the negative electrode, which allows a much easier observation of the parts at which the negative glow chiefly settles. I shall show that it tends to accumulate *away* from the positive electrode.

The glow which surrounds the negative electrode is divided into three layers which are, however, only clearly separated when the pressure has been reduced to the fraction of a millimeter. The first layer narrow, and closely surrounding the negative electrode, is with new electrodes of a beautiful golden colour. Its spectrum is chiefly made up of the hydrogen and sodium lines. The sodium is evidently due to matter which has settled on the metal, and the hydrogen comes out of the metal where it was absorbed. In time these lines disappear, the layer loses its golden colour, and the spectrum is now that of the positive half of the discharge. The second layer is the so-called dark space. The name is a good one, although Goldstein has pointed out that this layer is only relatively not absolutely dark. Its luminosity depends much on the gas used. With nitrogen it seems to me to be much darker than with the hydrocarbons. The thickness of this dark space is according to the measurements of Puluj about forty or fifty times as great as the mean free path in air at the same pressure. But the thickness depends on many circumstances, and would not, according to Puluj, be quite proportional to the mean free path at different pressures as we should expect it to be; but measurements of this sort can be only very approximate at present.

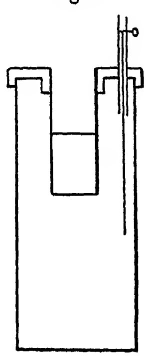
The third layer is the glow proper. When the curvature of the electrode is small its inner boundary is sometimes defined with quite a remarkable sharpness, but this also depends on the gas which is used. The outer boundary of the glow extends to different distances, according to the intensity of the current, the size of the electrode, and partly also its shape. It would be useless at present to enter further into the different causes which influence the thickness and definition of these different layers.

The glow is separated from the positive part of the discharge by a non-luminous space, sometimes also called the dark space. In order to prevent confusion I propose the name "dark interval" for it.

I have used in my experiments on the glow a cylinder open at one

end. Its internal diameter was 15 centims., its height 38 centims. The open end was closed by means of a brass disk into which a groove had been cut. The rim of the open end of the cylinder was cemented into this groove. A circular hole 5·5 centims. in diameter was cut into the centre of the brass disk, and another glass cylinder was fitted into this hole. The negative electrode was formed by a piece of aluminium foil 8 centims. long, which was wrapped round the inner cylinder. The electrode had a surface therefore of over 120 square centims. A long aluminium wire running parallel to the axis of the aluminium cylinder at a distance of about an inch formed the positive electrode. The aluminium cylinder was connected metallically to the brass disk, so that the positive electrode had to be insulated from it. This was done by passing it through a glass tube which was cemented into the brass plate. This tube also formed the opening through which the exhaustion could be carried on. The aluminium wire had, of course, to be connected with a platinum wire sealed into the tube. The accompanying figure shows a section of the vessel.

Fig. 1.



As an air-pump I have used Hagen's form of the Toepler mercury pump. As the vessel to be exhausted was rather large, the exhaustion was a little laborious, and had to be carried on with great care. I need hardly add that the effects to be described are not the result of a single experiment, but that the vessel was often refilled and exhausted in the course of them. I shall now describe what is observed on exhaustion.

At first the discharge, of course, only passes as a spark. It runs irregularly along the wire and passes to the nearest point of the aluminium cylinder. Gradually the glow settles down on the cylinder, if that is negative, as I shall assume it to be. The glow is quite irregular and not always at the same place, but a tendency to avoid the place opposite the wire is already apparent. In fact the effect

was first discovered at this stage of the exhaustion. The glow gradually becomes wider. At first it confines itself to the half-cylinder which lies towards the positive wire, but it is very irregular in shape. During the first exhaustion, it was interesting to notice how on one day there were some spots on the aluminium cylinder which were illuminated with especial brilliancy by the glow, while on the next the same spots were remarkable by the absence of any glow above them. All these effects depend of course on the state of the aluminium surface; gradually this becomes more uniform and the glow more steady.

On exhaustion the glow gradually covers the whole aluminium surface; but a dark strip about 2 or 3 centims. in width is observed directly opposite the positive wire. The appearance is well represented by fig. 4, Plate 4, representing a photograph taken of the phenomenon. I shall refer to the dark strip as the "dark area." As exhaustion proceeds, the glow extends further into the vessel. The dark space surrounding the cylinder becomes more apparent and expands, but the dark area loses its sharpness. Fig. 5 represents a photograph taken at this stage. The dark area increases slightly in width with decreasing strength of current.

When the cylinder is positive and the wire negative, very little is seen of the positive part of the discharge. At pressures of about a millimeter two reddish-yellow bands are noticed running parallel to the axis of the cylinder on either side, and symmetrical to the wire. They are too faint to be photographed. When the exhaustion is carried further these bands become less and less distinct, and gradually disappear.

I shall describe in another part of the paper some curious phenomena which are seen on sudden reversal of the current either in one direction or another, also the effects of a magnet inside the aluminium cylinder on the glow surrounding it.

Explanation of the Repulsive Effect of the Positive Electrode on Glow.

The following seems to me a plausible explanation of the phenomenon which I have just described. The rapid fall of potential which is observed on crossing the negative electrode suggests at once, independently of any theory, that we have to deal with the action of a condenser, for we know that no statical charge can produce a finite difference of potential at the electrode, while a double layer will produce a discontinuity. Although it may not be proved that an absolute discontinuity of potential exists at the kathode, it is yet certain that a very rapid fall takes place. This is all that is necessary for the argument.

We recognise such a double layer in the case of electrolytes, but

there is an essential difference in the thickness of the layer within which we must imagine that condenser action to take place. In the liquids that thickness must be very small, as is shown by the intensity of the observed polarisation currents. The positively electrified matter in every case is kept against the negative surface by a joint action of electrical and chemical forces, for it has been shown by Helmholtz that only thus can we explain a difference of potential between two bodies. It is the chemical forces which keep the two electricities asunder. The gaseous molecules or atoms, however, subject to their mutual encounters, and always having certain velocities, will tend to leave the surface. They are kept near the surface by the electrical forces. I do not, of course, mean to imply that the positive part of the condenser is always made up of the same molecules, but only that the time during which each positively electrified particle forms part of the condenser is large compared to the time during which it would be in the neighbourhood of the electrode if they were both unelectrified.

Suppose, now, that a positive electrode is placed near such a condenser. The resistance of the gas is so much greater than that of the metal electrode that we shall assume the whole electrode to be of the same potential. The lines of force will then cut the surface at right angles, and could we assume the condenser to be infinitely thin, there would only be a normal force acting on its particles; but as the lines of force are curved, the particles not in immediate contact with the surface are acted on by a tangential force which will tend to drive them away from the positive electrode. As a steady state will only be possible when the total force is normal throughout the condenser, we arrive at the condition for the steady state that within the condenser the fall of potential must be the same for equal distances measured along the normal to the surface.

Some idea of the distribution of potential may be gained by considering a positively charged point near two concentric spheres, the inner one of which is charged with negative electricity. The outer sphere representing the outer coating of the condenser will act as screen; the distribution on the inner sphere will therefore be uniform. The analogy is, of course, not complete, as we cannot assume the positively electrified particles to be distributed over a surface merely.

Our experimental evidence speaks in favour of the conclusion to which we have arrived, namely, that the fall of potential is equal for equal distances taken normally away from the negative electrode. This seems to follow at least from the fact that the boundary of the dark space is, whenever that is possible, equidistant from the electrode, for we know that the width of the dark space depends on the intensity of current. When the electrode is of such a shape that the normal drawn outwards from it meets its surface again, or whenever

negative electrodes are near one another, more complicated phenomena arise which we need not discuss here.

It is curious to observe the boundary of the dark space when the electrode is a cylinder. Round the surface of the cylinder the boundary of the dark space is a concentric cylinder. Opposite the end of the cylinder it is a plane, and the plane and cylinder are united by means of a surface, which is the envelope of spheres drawn with the same radius from the circular edge of the cylinder as centre, that is to say, by means of a part of a circular ring made up of quadrants. Goldstein has described similar forms of dark spaces. The point of interest, however, is this: that while on the cylindrical and plane surfaces the boundary of the dark space is well marked and bright, it is extremely indistinct on the circular ring. The amount of electricity crossing each part of the surface seems to be the same, while wherever the lines of flow separate the intensity of the current must decrease. We obtain in this way a smaller intensity of current at the annular surface.

Hitherto we have only assumed a certain number of particles positively electrified in the immediate neighbourhood of the negative electrode, and we have left it altogether undecided what these particles are. But if we consider now the fact that the glow does not appear opposite the positive electrode, that is to say, that while the fall of potential is the same all over the surface, the flow is stronger at some places than at others, we are driven to the conclusion that the flow does not altogether depend on the fall of potential, and we must again look for an explanation in the chemical as well as the electric forces. Wherever the fall of potential is chiefly produced by the presence of the positively electrified particles, which I now assume to be the decomposed molecules of the gas, these will help by their chemical action to decompose other molecules. Opposite the positive pole the fall of potential is principally due to nearness of that electrode; chemical forces are absent, and the molecules will not be decomposed. This is, I believe, the explanation of the dark area. And it brings with it the explanation of a large quantity of other facts, as, for instance, the one which has been so long observed and well established, that once a current is set up in the gas it requires a much smaller electromotive force to keep it going. For the discharge, according to us, will generally be introduced by a spark which must give the first supply of decomposed molecules before the continuous glow discharge can establish itself.

The effect of an increase of current on the dark area is also easily explained, but I do not propose to go into details in the present paper.

If my explanation is true, we must expect the glow to be strong between two negative electrodes, and that is the case.

I may for the sake of clearness once more mention shortly the principal points of the argument.

The rapid fall of potential in the neighbourhood of the negative electrode renders the presence of positively electrified particles in its neighbourhood necessary.

If the distance through which the condenser action takes place is sensible, the positively electrified particles will be acted upon by a neighbouring positive electrode.

A steady state will be established in which the fall of potential along the normal from the surface will be everywhere the same.

As however the flow is stronger away from the positive electrode, we must conclude that other forces besides electrical forces determine the flow.

It is natural to assume that these are chemical forces: that in other words the positively electrified particles are the decomposed molecules, which by their presence assist the decomposition of others, and therefore the formation of the current.

Unless a flaw is detected in this line of argument, I think that the conclusion must be granted, namely, that the decomposition of the molecules at the negative electrode is essential to the formation of the glow discharge. This is really all that I endeavour to support in this paper. The rest can only be settled by further experiments. And amongst the rest I count also the primary cause which originally produces the decomposition of molecules at one pole rather than at another. It is possibly due to an electromotive force of contact between the gas and the electrodes which tends to make the gas electronegative.

It does not seem difficult to explain by our theory the phenomena which happen at the negative electrode on exhaustion. When the pressure is high the discharge passes in a series of distinct sparks, separated by a sufficiently long interval of time to make each spark independent of the one preceding it. Here, of course, the spark will set out from that part of the negative electrode at which the tension is strongest. As the vessel is gradually exhausted the sparks succeed each other more rapidly, and the molecules decomposed during one discharge assist the next discharge. If the decomposition of the molecules goes on at a sufficiently rapid rate the tangential action of which I have spoken comes into play, and the negative glow will cover a greater part of the negative electrode. The discharge at the same time can become continuous, for the state of polarisation near the electrode can keep up a continuous stress. The stronger the current becomes, the more easily will the negative electrode become covered with the glow.

The Dark Space and Glow.

I enter now into a more detailed account of the phenomena which happen in the neighbourhoood of the negative electrode. We consider this electrode to be surrounded by a layer of electro-positive particles. The molecules are decomposed partly by chemical and partly by electrical forces, and the electro-negative part will be able to follow the forces acting on it, and acquire a considerable velocity within a small distance. This velocity will gradually be reduced by impacts, and the temperature thereby raised: hence the luminosity of the glow. The dark space must, therefore, be considered as the region through which the greater number of atoms can freely pass. But, as already observed, the dark space is itself slightly luminous, as it should be. We cannot, of course, know anything at present as to the mean free path of the constituents of the decomposed molecules, especially as they move in a non-homogeneous atmosphere, traversed from one side by the molecules coming from the positive pole, and from the other by the products of decomposition. The chief difficulty seems to me to be the explanation of the sharpness of the boundary of the dark space in certain cases.

Mr. Goldstein has described some interesting experiments under the title of "A New Kind of Electrical Repulsion." I think that his experiments admit of a very simple explanation. When the glow is allowed to fall on a screen through which a small aperture is cut, its rays are seen to be propagated in straight lines from the aperture. If such a ray passes close to another negative electrode it is deflected. This seems to me to be a necessary consequence of the fact that the potential in the neighbourhood of an electrode alters very rapidly, and that therefore strong forces must act on a passing particle charged with negative electricity. Mr. Goldstein finds that as long as the two kathodes are metallically connected the effect is the same, whatever the pressure of the gas, whatever the nature of the gas, and whatever the strength of the current. All these laws are easily deduced from our theory. The velocity is acquired in the immediate neighbourhood of the negative electrode; if the fall of potential is increased in a given ratio the square of the velocity is increased in the same ratio, and throughout the path all the forces are increased also in the same ratio. Hence the path must be the same, and as neither the amount of electricity carried by each particle nor its mass would affect the result, the total deflection of the ray is independent of the nature of the gas.

Goldstein claims to prove that the repulsion is not exerted through solid substances like glass or mica, but his experiment admits of a better explanation. According to Hittorf we must consider the glow to be a good conductor, that is to say, metal wires which are placed

inside the glow readily take up the potential of the surrounding space. A screen of glass, therefore, in front of an electrode will become covered with electricity until the potential in its neighbourhood does not vary in a normal direction, and if the ray from a negative electrode passes tangentially, there can be no deflective force on the particles which make up the ray, the piece of glass will act as an electrical screen. We shall never be able to show for the same reason any deflecting force due to a statically charged body, for if the body is simply placed inside the vacuum, near enough to influence the current, it will discharge itself, and if it is covered with some non-conductor, that non-conductor will become charged on its outside until its effect on the lines of force has been counterbalanced.

A serious objection against Mr. Crookes' view that particles are projected from the negative electrode with great velocity has been first urged by Mr. E. Wiedemann, I believe, and was afterwards repeated by Goldstein. It would apply with equal force against the theory which I am defending. Particles moving with great velocity in one direction should send out light, the wave-length of which is affected by the motion. No such effect can be observed, although Goldstein has undoubtedly proved that the velocity must be sufficiently large to make it perceptible. But the particles moving with great velocity are not themselves luminous, that is proved by the existence of the dark space. It is only when their velocity has become sufficiently reduced and the energy is distributed in all directions, that the particles are luminous; but then we do not get a one-sided displacement, but only a very small widening due to the motion of the particles in all directions.

Mr. Crookes has given reasons why we should consider the region of the dark space as one in which directed motion prevails, and although Hittorf has raised serious objections against the arguments drawn from his radiometer experiments, which seem to be explained by secondary temperature effects, the general conclusion which he has drawn from his experiments is not thereby invalidated, for the rise of temperature itself requires explanation.

Proposed Test of the Theory.

The most conclusive proof of our theory would be the demonstration of the fact that each particle of matter carries with it the same amount of electricity. We shall not, of course, be able to prove this for each single particle, but I propose to show how we can decide the point experimentally as far as the average amount is concerned. Suppose a small straight beam is cut out of the glow and placed in a field of uniform force, the lines of which cut the rays of the glow at right angles. The force being everywhere normal to the rays, these will curl up in a circle. This has been shown to be true experimentally

by Hittorf. I think a careful measurement of the radii of such circles will give us important information, and I have already made preliminary experiments which have shown me that such a measurement is possible.

The force exerted on each part of the current is proportional to $v \times e$, where v is the velocity of each particle, and e the amount of electricity it carries. If the particle moves in a circle the force is also proportional to $\frac{v^2}{r}$ where r is the radius of the circle. Hence r must be proportional to v/e.

Suppose, now, that the current is increased. This may mean either that the number of particles carrying the discharge is increased, or that the velocity of each particle is increased, or that the amount of electricity is increased, or, finally, some combined effect of these three causes. When we come to consider the positive part of the discharge, I shall show that the number of particles taking part in the discharge seems certainly increased by an increasing current, and this is also evident from the mere appearance of the discharge; this does not, however, affect the influence of the magnet. I find as a result of experiment that the diameter of the rings is considerably increased by an increase in strength of the current; hence an increased current must either mean a larger velocity only, or, at any rate, a velocity increasing in a quicker ratio than the amount of electricity carried. Before going further we must consider in what way v depends on e. Let the total fall of potential in the region in which the velocity is acquired be F, than v will vary as the square root of Fe, and hence the diameter of the ring will vary as $\sqrt{\frac{F}{e}}$. If e is constant the diameter of the ring ought to vary as the square root of the fall of potential in the neighbourhood of the negative electrode. Here, then, we have a definite experimental problem before us which I hope to decide one way or another as soon as I have the necessary experimental means at my disposal. At present I wish only to point out that if e does vary there is every reason that it cannot vary otherwise on the average than proportionally to F. Hence the diameter of the ring ought to be independent of the current, which it is not.

Experiments with Hittorf's Tube.

Mr. Hittorf has described some phenomena which are, in my opinion, particularly interesting, as putting beyond all reasonable doubt the projection of particles away from the negative pole.

The tube, or rather the bulb, which he used had two parallel electrodes at a distance of only a few millimetres. It was found that at very low pressures the discharge from the positive electrode took

place not towards the negative pole, but in the opposite direction. I have repeated these experiments with electrodes which were rather further apart. The behaviour of the tube under different circumstances can be shortly expressed by saying that the discharge always passes to the nearest point of the inner boundary of the dark space. When the exhaustion is not sufficient, so that the width of the dark space is less than the distance between the electrodes, the positive discharge takes place towards the negative pole as in the ordinary tubes, but as the dark space gradually expands, the positive discharge contracts, and becomes invisible when the dark space comes into contact with the positive wire. On further reduction of pressure the dark space reaches beyond the positive pole, and the discharge passes from that pole to the nearest point of the negative glow. In other words, the tube behaves as if the glow, and not the negative wire, formed the electrode. But this is exactly what should happen according to our theory.

The negative particles which are projected from the wire have first to be brought to rest by molecular encounters before their motion is regulated by the electrical forces in the ordinary sense.

The experiment is conclusive to my mind, because in the neighbourhood of the positive discharge, as it turns away from the negative pole, we have the current flowing in two opposite directions at closely adjoining places. This could not be possible unless the current in one direction was carried by particles moving against the lines of force by their inertia. The positive discharge at the lowest pressures which I am able to obtain shows some curious phenomena, indicating perhaps that to some extent the phenomena of the negative pole are repeated to a much smaller degree at the positive pole. A dark line is noticed parallel to the positive wire at a distance varying with the pressure, and at the best exhaustion which I could obtain about 1 millim. distant from it. From the wire to the dark line the discharge is very narrow, but from the dark line it spreads out fan-like towards the negative glow.

Fig. 6, Plate 1, is taken from a photograph, and shows the phenomena which I have just described. As far as the negative glow in these tubes is concerned, the repulsive forces from the positive pole make themselves very perceptible, so that the glow becomes stronger away from the positive wire, and the tube presents the curious appearance of two discharges from the two poles tending away from each other and towards the glass vessel. The current completes itself on the side of the vessel. This is clearly shown in a photograph (fig. 7), which represents a projection in a plane at right angles to the wire poles. These poles appear therefore as points, and the two discharges passing away from each other and joining over the glass are clearly distinguished.

Effects observed on Reversal of the Current.

I now pass to a short description of some curious effects observed on sudden reversal of the current. In my large vessels in which the dark area opposite the positive wire appeared, the phenomena seen when the wire was negative and the cylinder positive present little that needs mentioning in detail. The only trace of positive light is confined to two yellowish lines running parallel and symmetrical to the wire. We have here very likely to deal with the reaction to the repulsive effect on the glow. If, when these yellow bands are seen, the current be slightly interrupted for a short time and passed again in the same direction, these bands will appear perfectly steady from the beginning of the passage. If, however, before making contact in the same direction, a single spark be passed in the reverse direction, the two bands will at first appear close together and right opposite the negative wire, then quickly to move outward and to take up their final position. If, after the cylinder has been positive, the current is interrupted for a longer period of time, say five minutes, then the same effects as on reversal will appear. Thus it seems that a certain state has been established which dies away in something like five minutes, but which can also be made to disappear by a single spark in the opposite direction.

Similar effects, though not so striking, are observed with the wire negative. The dark area seems to take time to establish itself. A short interruption of the current will make it reappear fully from the beginning, but if either five minutes are allowed to lapse, or a spark in the opposite direction is passed through the vessel, the dark area will not be well developed to begin with, but is filled with a faint glow which, however, dies away rapidly. These phenomena are, I think, fully in accordance with the theory here suggested, for it is clear that the condenser action will take time to establish itself and to die away, just as in a liquid. The effects observed when the cylinder is positive are not, in my opinion, produced by such a condenser action at the positive pole, but are secondary effects depending on the changes in the glow. When the current is finally established, the glow will be strongest away from the cylinder, and the positive discharge will consequently set out from parts of the cylinder not directly opposite the negative electrode. Hittorf has already noticed that when the negative electrode consisted of a long wire, the glow was found on first making contact to start from the point and to run backwards, covering gradually the whole of the wire. This is perhaps the place to dispose of an objection which might be raised against the theory. There are apparently no appreciable polarisation currents like those observed in liquids, but I do not think that this is a very serious difficulty. We usually represent

the action which takes place at the poles in an electrolyte as a condenser action, and we can calculate from the measured capacity of the condenser the distance between the two charges of electricity. In the case of liquids, this distance is extremely small, and is given by Helmholtz in his Faraday lecture as the ten-millionth part of a millimetre. Though the total fall of potential in a gas which is measured by the moment of the condenser might be the same as, or even much stronger than in the liquid, the charges might easily escape detection, if the distance between the layers is say ten thousand times larger in the gas than in the liquid. The phenomenon of the dark area tends to argue in favour of a greater thickness of the region between which the condenser action takes place.

The Positive Part of the Discharge.

I cannot enter in the present paper fully into the phenomena which happen in the positive part of the discharge. It is too early as yet, I think, to form a distinct idea as to how the electricity is conveyed in the manifold forms which the discharge seems able to take. But a few remarks may not be out of place to show that we meet with nothing that is contradictory to the theory which I have put forward. A remarkable result obtained in different ways by Hittorf and by E. Wiedemann is on the contrary rather in its favour. Hittorf has found that the fall of potential in that part of the tube which we have for convenience' sake called the positive half, is independent of the intensity of the current passing through it. This means that the current into the resistance is a constant quantity for a given tube and pressure, or in other words, that the energy dissipated is directly proportional to the current, and not to the square of the current as in a metallic conductor. It had, indeed, been found previously by E. Wiedemann that the heat developed in a series of discharges is proportional to the quantity of electricity which has passed through the tube, and independent of the fact whether that quantity has passed in a few strong sparks or in a greater number of weaker ones. This fact seems to point to the conclusion that in the positive part of the discharge a greater intensity of current only means that a greater number of particles takes part in the discharge, but that the velocity of diffusion of the particles carrying the discharge is independent of the intensity of the current.

With regard to the stratifications, I should offer the following preliminary explanation, but only with the view of showing that the theory is capable of dealing with these phenomena. In the first place stratifications are seen at their best with compound gases, and also with mixtures of gases which are likely to form compounds. That the decomposed particles at the negative pole are liable to form

compounds is shown by the peculiar spectrum of the glow in many gases. Such a compound will behave as an electrified molecule, and travel towards the positive pole, the fall of potential will increase its velocity, and that will again be reduced by molecular encounter, the heat thus generated will possibly decompose the molecule again; we have then two ways in which the discharge can be carried, either by the electrified molecules or by the decomposed atoms. If, owing to some reason, the decomposing of molecules takes place at definite places in the tube instead of being distributed unequally throughout the tube, we have what practically becomes a secondary negative pole with its dark space. There is much to be said in favour of the view that the intervals between two stratifications are due to the same causes which produce the dark space at the negative electrode. It is needless at present to speculate on the original cause which fixes the positions of these secondary poles.

I can see nothing antagonistic to our views in the secondary negative poles discovered by Goldstein to exist wherever the width of the tube rapidly widens in the direction from the negative to the positive electrode.

The Influence of the Magnet on the Electric Discharge.

The influence of the magnet on the electric discharge is generally supposed to be well understood, but a good deal yet remains to be said on the matter, for although we can in each individual case trace out pretty well why a certain effect happens, it is yet impossible, according to the results given hitherto, to predict beforehand the behaviour of a vacuum tube in the magnetic field.

The magnetic influence on the positive part of the discharge is either compared to the effect on an elastic thread carrying a current, or looked on as an effect on particles projected with great velocities from the positive pole. I do not think that either of these views gives a correct representation of the matter.

Let us compare for a moment the effects which are observed when a magnet acts on currents passing through liquids and gases. Supposing a liquid cell is placed equatorially in a magnetic field, strong movements of the liquid are observed, yet the lines of flow of electricity seem to be unchanged. I consider that the first effect of the magnet on a current through a gas is exactly the same; but then comes the difference. The particles of the gas which have carried the discharge conduct better than those which have not, and consequently the spark continues to pass through the particles which have been thrown aside by the magnet; that is to say, the current becomes deflected. As the current continues it will become deflected further and further, yet there will be a tendency to fall back into the original line of least resistance. The final form of the discharge will there-

fore not be one of equilibrium, but a compromise between the continuous rotation which the magnet tends to set up and the tendency to choose the path of least resistance. There is a real difference between this view and that generally brought forward; for, if I am correct, we should have in the final stage, which to us seems to be one of equilibrium, the particles of the gas carried through the current in a direction at right angles to it.

I think that I may quote the experiments of De la Rive on the rotation of a current round a magnet in support of what I have said. I have repeated his experiments by using a vessel very similar to those previously described: the inner cylinder contained the pole piece of a strong magnet, and was not used as electrode. One of the electrodes was a ring surrounding the axis; the other, a wire in the direction of the axis of the magnet. It has already been pointed out by De la Rive that the experiment is not successful with air; there seems a tendency towards rotation, but no rotation. Here, then, the preponderance of conductivity of the particles which have already carried the discharge does not seem sufficient to rotate the current; yet we cannot imagine that, for the same intensity of current, the effect of the magnet should not be the same. We must conclude, therefore, that here the particles are driven through the current, but do not carry the current with them. Even with vapours like alcohol, with which a continuous rotation can be obtained, it often happens that the current rotates only through a small angle, then suddenly jumps back into its original lines, and then tries to go round for some time, until it finally succeeds. Those who are familiar with the experiments of Plücker will find, I think, that they can easily be explained by what I have said, but that in no case can the form in which the discharge takes place be one of equilibrium; nor is it even a circle, which is the curve in which a flexible wire carrying the current would set if it was fixed at both ends.

As far as the effect of the magnet on the negative glow is concerned, it is generally considered sufficient to say that it sets in magnetic lines of force, or sometimes that it winds itself in spirals round those lines. The facts are true, and easily explained by any theory of projection, but they do not contain the whole truth.

In order to understand fully the action of the magnet, we have to be careful in distinguishing two totally distinct questions. The first question is this: In what way does the magnet affect the rays of the glow which start from any given part of the negative electrode? This question has been satisfactorily answered by means of the theory of projection. The second question, which has not received sufficient attention, is this: At what parts of a negative electrode placed in magnetic field does the glow form itself? For we find that large parts of a kathode sometimes remain absolutely dark in a magnetic

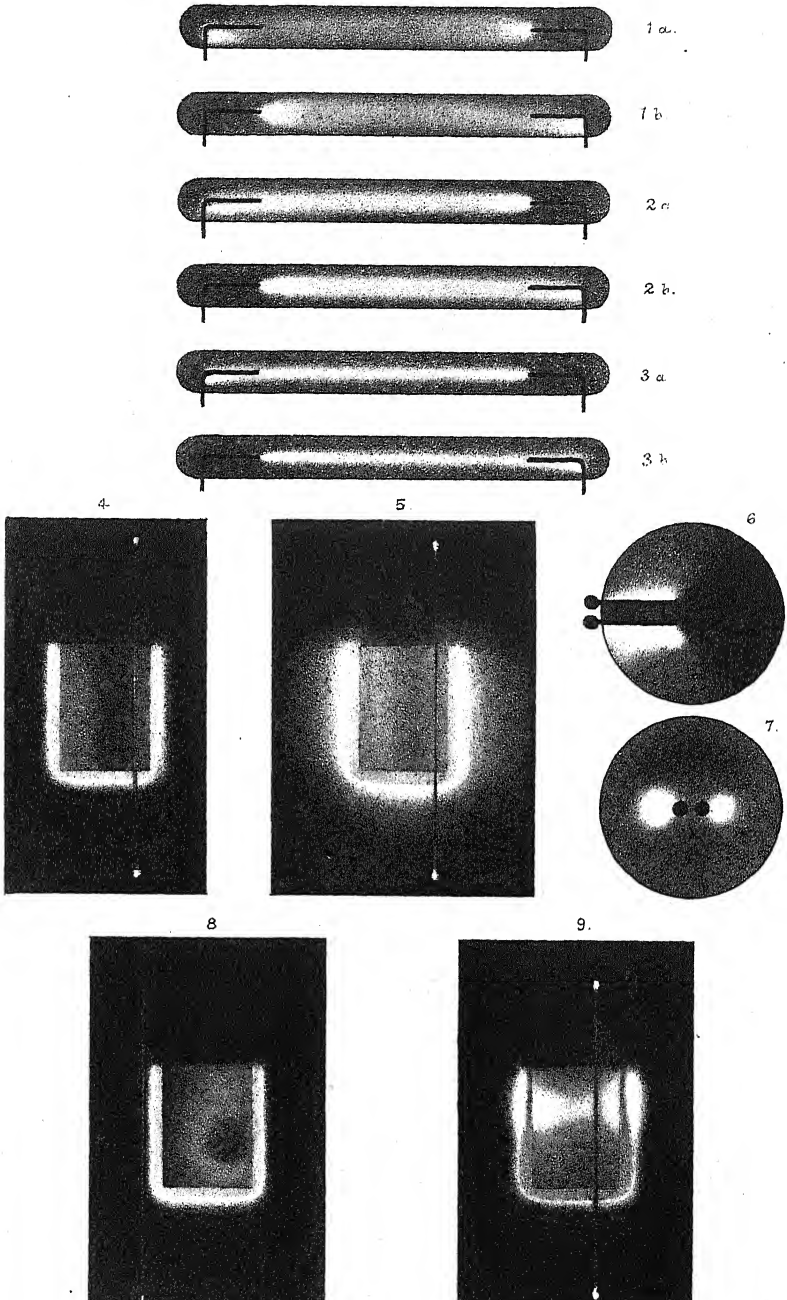
field, while they are covered with a glow on removal of magnetisation.

I have found the following explanation satisfactory in accounting for the phenomena observed by me or recorded by others. Suppose the whole negative electrode to be covered with the glow and then placed into the magnetic field. The negatively charged particles will be deflected in their path, and at some times they will tend to separate, at others they will be thrown together. Where they are thrown together the temperature will rise, and the discharge will pass more freely, and the current here will be strengthened, and the temperature still further increased. There is then, finally, a tendency of the glow to settle down *exclusively* at those places at which, owing to the effect of the magnet, the particles are thrown together. If in the vessel (fig. 1) a small electromagnet is introduced into the inner cylinder with its axis at right angles to the axis of the cylinder, it is found that the parts of the aluminium foil covering the two poles of the magnet are absolutely dark (fig. 8, Plate 1). It is clear that, if there was any glow at these parts, the particles carrying the current would be driven asunder, while they are thrown together near the middle parts of the magnet, where consequently the glow becomes strongest.

If into the same vessel we place a magnet longitudinally, the effect is that observed in fig. 9. Here again the glow settles round the centre of the magnet. The electromagnet used in this last experiment had a length of 8 centims., so that it filled completely the aluminium cylinder. That the question here introduced as to the parts of the kathode at which the glow chiefly settles is distinct from the more direct effect of the magnet on the glow, once that glow is formed, is clearly shown in cases where the vessel contains two negative electrodes, one of which only is affected by the magnet. I have used, for instance, a vessel containing two aluminium cylinders opposite each other. If a magnet is introduced into one of them, the glow will sometimes be taken away entirely from the other cylinder; a fact easily explained by the greater facility of the formation of the glow where the temperature is increased by a concentration of particles.

Fig. 9 also shows a curious effect of the magnet on the width of the dark space. Where the glow is strongest the dark space is very narrow, while it is widened wherever the glow is weak. It has been proved by Crookes that the dark space becomes narrower when the intensity of the current is increased, and the effect shown in the photograph is a simple consequence of this fact.

I have been during the whole of this investigation very ably assisted by Mr. Arthur Stanton; without his help I should not have been able to overcome the great practical difficulties which present themselves



W. H. Wesley lith. West, Newman & Co. imp.

when large vessels of complicated form, like those used by me, have to be put together and exhausted. All photographs were taken by him. I have also to acknowledge valuable assistance from Mr. Moss, a student at the Owens College.

"On the Experimental Determination of the Index of Refraction of Liquefied Gases." By Dr. L. BLEEKRODE. Communicated by Dr. GLADSTONE, F.R.S. Received and read June 19, 1884.

I. *Preliminary Remarks.*

On a previous occasion* I had the honour to present to the Royal Society some results of an investigation made about electrical conductivity of chemical compounds, and I then chiefly examined the liquefied gases, pointing them out as very bad conductors. I have since been engaged in studying another property of these substances, and I have succeeded in determining in an experimental way their refractive power. As in England and abroad several papers have been subsequently published bearing on the relation between the liquid and gaseous conduction of matter, and especially liquefied gases present themselves well adapted to this kind of research, I hope the Royal Society will consider my paper not devoid of interest, the more so because our knowledge of their physical constants is somewhat limited. And though we possess numerous determinations of the refractive power of a vast number of chemical compounds, still increasing daily, I have found only very little information concerning my subject, this being limited to sulphurous and prussic acid, that are readily liquefied by cold and present no difficulty in manipulating. Faraday in his extensive paper on liquefied gases, published in 1823, when describing the properties of several of them, compares only their index of refraction to that of water, calling it more or less, and Brewster in 1826 mentioned in a communication to the Society of Edinburgh the index of refraction of liquefied cyanogen as 1·316, but without any remarks on the manner in which it was deduced.

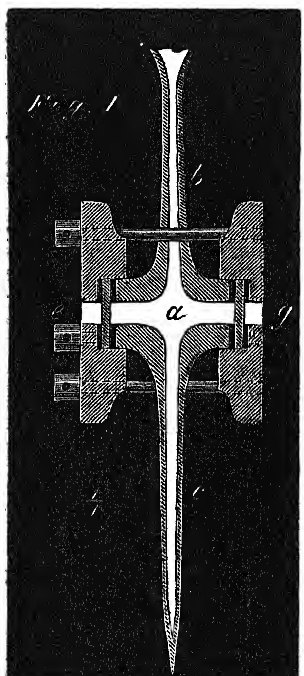
I will commence this paper by describing the method I followed to obtain the numerical values of the index of refraction of several gases, liquefied either by pressure or by cold, and that enabled me to surmount the difficulties resulting from high tensions and small quantities of fluid substance, that may perhaps have kept back other experimenters from this field of research.

* "Proc. Roy. Soc.," vol. 25, p. 322.

II. *Description of the Apparatus.*

Several methods are now at command in physics by which the index of refraction may be determined even to the sixth decimal, yet in this peculiar case, from the very conditions in which the liquids were to be examined, it was not safe to employ prisms having glass walls cemented on, as cements are often soluble in the liquefied gases; besides rather great dimensions would have been required, rendering the effect of pressure the more difficult to resist.

As in my former investigation, I chose again in most cases the Faraday tubes, but I had them modified in the proper way, so as to obtain the liquid inclosed between parallel glass sides, when I could apply the microscope to make quantitative experiments.



After many preliminary arrangements that were successively tried I came to an equally effective and simple construction, that showed itself most convenient and is represented in figs. 1 and 2, and I suppose it may prove useful also in other researches with liquefied gases. A section of the apparatus is given in fig. 1; it consists of a short glass cylinder a (length 15 millims., inner diameter 3 millims., outer diameter 11 millims.), communicating on both sides

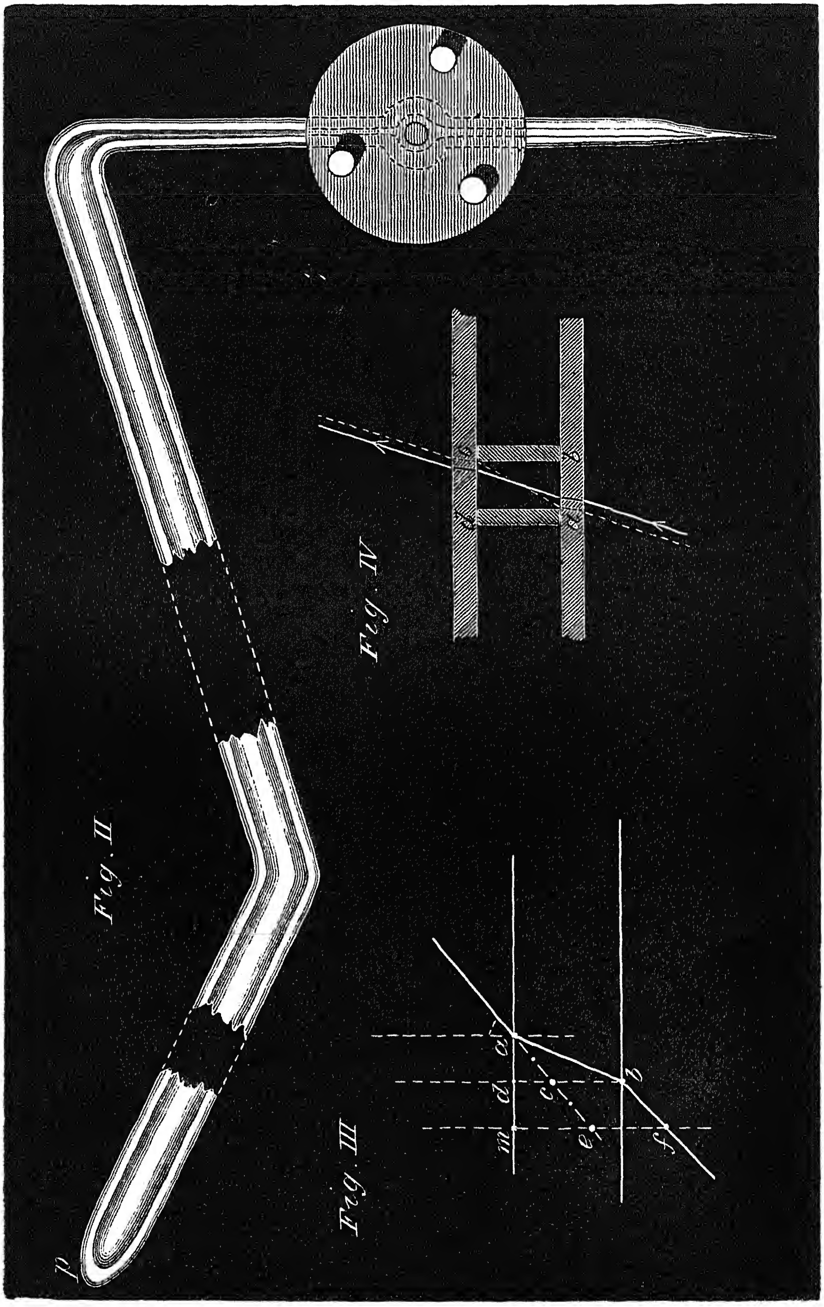


Fig. II
p
Fig. III
a
b
c
d
e
f
m
Fig. IV

with capillary tubes *b* and *c*; this was transformed into a vessel having two parallel side plates, and free from leakage even at high pressures, in the following manner.

The ends of the cylinder were both ground as flat as possible, on each of them a thin circular disk pierced in the centre and cut from lead or tin, according to the liquid examined, was put, subsequently a plane glass plate that had been previously submitted to the toughening process of La Bastide (in some cases a quartz disk was used) followed, and lastly a perforated smooth leathern disk. The whole was held together by two perforated steel plates forming the ends (fig. 1 shows the construction, *e* and *g* showing the apertures) and connected with another by three screws turned on as strongly as possible.* A perfectly gas-tight vessel was thus obtained that did not allow leakages even at pressures above a hundred atmospheres, as was tried with the compression apparatus of Cailletet. And I must point out here especially the property of toughened glass to resist in the most effective way the always more or less unequal pressure, exerted by the three connecting screws, when even quartz plates being 3 millims. thick burst to pieces, if the screws happened to be turned on unequally. The glass plates, having only a thickness of 1 to 1·5 millims., were cut out of very plane surfaces, and after being reduced to the proper dimensions, suffered the toughening operation, that was applied with much care. Of course some of them got their surface more or less impaired and rendered less transparent; from a great number the best ones were chosen, and they never became unfit for use.

In most cases the lead disks, procuring a very successful closing, were not attacked in a chemical way by the liquefied substance if no moisture accompanied it (with hydrobromic acid tin disks resisted better), and thus commonly a perfectly clear liquid filled the cylindrical vessel.† In how effective a way leakages were prevented may be concluded from the fact that liquid carbonic acid was often kept during more than a year in such hermetically closed vessels, and even when heated during several hours above 30° C., without a perceptible loss occurring.

If the gas could be evolved chemically in a sufficient quantity to become liquid by its own pressure exerted, the cylindrical vessel was fixed with the blowpipe on a stout bent glass tube (fig. 2), having a length generally of 40 centims., an inner diameter of 6 millims., an outer diameter of 10 millims. The appropriate substances were put in the

* The steel plates were 8 millims. thick; in the figure only two screws could be represented entirely.

† Organic substances, applied to obtain a gas-tight closure, should be avoided when working with apparatus for liquefied gas, as these are commonly absorbed by the liquids, rendering them often opaque.

lower part of the tube, in the upper one some compounds fit to absorb any moisture present in the evolved gas, and both ends sealed with the blowpipe. Afterwards the substances were put in contact by gently shaking the tube, and the operation conducted so as to have the liquefied gas distilling over into the vessel, directed with its axis in a horizontal position. Enough liquid was always obtained to fill it completely and generally also the connected capillary tubes on both sides, and no gas or air remained ; the small dimensions of this part of the apparatus caused some drops of liquid to be sufficient. The liquefying operation conducted with the Cailletet pump proceeded in the usual way; the capillary part *b* (fig. 1) of the vessel was fixed with the blowpipe on the ordinary gas tube of this apparatus, the other capillary part *c* was closed after this tube was filled with the purified gas.

If the gas could be put readily into the liquid condition with the aid of low temperatures, the cylindrical vessel was put on a doubly bent glass tube, and this surrounded by a freezing mixture; this was applied in the case of sulphurous acid, some amine compounds, &c.

Lastly, I will observe, that after terminating a series of experiments with some gas, the whole apparatus could be liberated from its contents by cutting off a very small part of the lower end of the tube, and all remaining substances were removed by the pressure of the gas; thus I could afterwards examine the glass disks and convince myself whether the liquefied gases, being perhaps not pure, had left behind some moisture or other deposits.

III. *The Method of Observation.*

The reason why I had to provide for getting the liquefied gas inclosed between parallel glass plates was to have an opportunity to apply the microscope, which perhaps may be the only practical effective method under these circumstances.

The use of this instrument in determining the index of refraction was first proposed by the Duke de Chaulnes in 1767, but the exactitude of the results being limited to the third decimal (from the principle on which the method is based) it is inferior to many other methods, and was, of course, neglected in physics.* The mathematical expression for the index that has to be used in this case follows from very simple considerations.

* In 1849 Bertin published some modifications of this method, without improving, however, its exactitude, as appears from the values communicated for the index of glass (1·497 and 1·703), the goniometer giving 1·507 and 1·696.

More recently, in 1876, Royston Pigott, when describing his refractometer, commented on adapting his instrument to the microscopical method, and in 1878 Mr. Sorby, of Sheffield, made several valuable and interesting communications on the refractive indices of minerals, which were determined with the microscope, and he also pointed out the practical use in the case of small quantities of liquids.

Suppose in fig. 3 a liquid layer of a certain thickness limited by parallel surfaces, its refractive power shall be such that any object placed in b will be seen apparently in c; hence the relation—

$$\frac{db}{dc}=\frac{\tan i}{\tan r}=\frac{\sin i}{\sin r}=n,$$

when the angles of incidence and refraction are very small, calling the thickness of the layer D, and the apparent displacement of $b=d$, the equation is obtained—

$$n=\frac{\mathrm{D}}{\mathrm{D}-d},$$

which gives the value of the index by measuring two linear dimensions.

Practically the case is not so very simple as is supposed, because the terminal glass plates, transmitting the rays, contribute to the displacement, as is shown in fig. 4. The effect caused by these two disks together I call the "glass effect," and it had to be determined for each vessel itself before it was filled with liquid; the amount was afterwards deducted as a constant correction. In fig. 4 the vessel is represented by *abdc*, and it is obvious that the rays, which can be transmitted through the liquid under the greatest angle of refraction, have a direction along *ac*, when they will pass through the apertures in the centre of the steel plates laid on *ab* and *cd*.

In the greater part of experiments, *ab* and *cd* measured 20 and 21 millims. respectively; the angle of refraction deduced from its tangent will not surpass a value of $5\frac{1}{2}°$, and therefore the error, caused by taking the sine instead of the tangent, only amounts to one unit in the third decimal.

The numerical values for D and d are easily obtained with the aid of the microscope, which was specially constructed for my purpose by Nachet of Paris. The ocular tube (that in most observations was placed in a horizontal direction) was provided with a vernier, moving along a scale divided in halves of a millimetre, and allowing to determine a displacement of the tube to $\frac{1}{20}$ millim.; a micrometer screw could also be used for a more accurate adjustment to the $\frac{1}{100}$ millim. Of course, the exactitude that may be reached in focussing any object depends on the magnifying power applied; unfortunately, I was limited in using very high powers by the necessary dimensions required by the glass cell to be strong enough and fit to the end in view. I chose a high magnifying ocular power and a very feeble objective lens, getting by this combination a magnifying power of nearly 30; it gave the advantage of having very well-defined and clear images to focus on of microscopic test-objects—as, for instance, the scales of wings of Lepidoptera fixed on the glass plates of the cell merely by adhesion on the inner surfaces. When the cell was constructed, I

began with determining its height, putting it before the objective lens of the microscope, and focussing first the objects on one of the glass plates, then those on the other; taking now the difference between the two readings the value of D was obtained. I remark that in this way the effect of the front glass plate (next to the objective lens) and also of absolute errors in the division of the scale and micrometer screw were eliminated. Putting successively the tube on other parts of the scale, I could take a range of readings numbering six or eight, from which the mean value was deduced.

The determination of d, the apparent displacement, was somewhat more complicated, a part of it being caused by the refraction of the glass walls of the cell, and this was to be first ascertained. I found it most convenient for this purpose to have the objects to be focussed not on the glass plates of the cell, but on a separate glass slide placed immediately behind it; otherwise in taking different readings, when working with a liquefied gas, I should have had the cell to be alternately filled and emptied again. Yet the deduced mathematical expression may also be used in this modified case. Considering in fig. 3 an object f placed beyond the cell to be focussed through it, either empty or filled, then, by the effect of refraction, it will be seen apparently in e; and when the angle fba is very small,* abf will be almost a straight line, and again the equation is obtained—

$$\frac{mf}{me}=\frac{db}{dc}=\frac{\text{D}}{\text{D}-d}=n.$$

Therefore, when a complete observation of n for any liquefied gas was to be made, a glass slide with test-objects was placed in front of the microscope, the glass cell still empty (whose height had been previously ascertained) followed close to it, and, putting the tube of the microscope in a horizontal position, I focussed the objects first through the cell, then, moving this aside, I focussed again; a set of such observations gave the mean effect of both glass plates of the cell together. Now the liquefied gas was prepared in the glass tube connected with the cell, and I took great care to have it completely filled without any remaining gas-bubbles. I obtained this easily enough, when maintaining the axis of the cell in a horizontal direction.

To get the index of refraction of the liquid, I focussed through it the test-objects; I determined the position on the scale of the microscope, corrected it for the glass effect, as described; moving the cell aside, I focussed again the test-objects directly; the difference between the former and the latter reading gave the value of d, and the height or D being already known, n is calculated. This method is dependent on the condition that the cell is provided with parallel

* With the greatest value of the angle of refraction, $5\frac{1}{2}°$, and with an index of refraction ranging between 1·2 and 1·3, the obtuse angle amounts to $178°{\cdot}9$.

sides; it was therefore necessary to ascertain for each experiment what reliance could be placed upon it, the more so because, though the glass plates themselves might have been very parallel, yet by a more or less unequal compression of the intermediate leathern and leaden disks, a somewhat prismatic form of cell could have ensued. Whether this were the case, I always controlled with every new prepared tube by trying to determine in it the index of refraction of sulphuric ether, in the same way as had to be done afterwards with liquefied gases. I compared the value obtained to that which has been found with the more exact methods; if it agreed within the degree of exactitude that comports the use of the microscope, I was sure that no error was to be feared from want of parallelism between the ends of the cell;* if there was discordance, I reconstructed the cell, till a satisfactory result was got.

To appreciate the influence of errors of observation on the obtained value, a differentiation with respect to D and d of the relation $n=\frac{D}{D-d}$ will give

$$\frac{\delta n}{\delta D}=\frac{d}{(D-d)^2}, \qquad \frac{\delta n}{\delta d}=\frac{D}{(D-d)^2}.$$

If a magnifying power of 30 were applied, I could adjust the test-objects with a displacement to 0·04 or 0·05 millim., calling the errors made in the observation e and e_1, the following expressions for the errors in the values of D and d ensue—

$$E=-e\times\frac{d}{(D-d)^2}, \qquad E_1=e_1\times\frac{D}{(D-d)^2}.$$

In an experiment with sulphuric ether, for instance, D measured 15·92 millims., d=4·18 millims., e=0·04 millim., e_1=0·04 millim. therefore,

$$E=-0{\cdot}04\times\frac{4{\cdot}18}{(11{\cdot}74)^2}=0{\cdot}0012;$$

$$E_1=\ 0{\cdot}04\times\frac{15{\cdot}92}{(11{\cdot}74)^2}=0{\cdot}0046.$$

In the most unfavourable case, the total error may amount to 0·0058, and probably it may be expressed by $\pm\sqrt{E^2+E_1^2}=\pm0{\cdot}0048$.

A higher magnifying power—as, for instance, one of 200 times—would have allowed of reading a displacement to 0·01 millim.; but mechanical difficulties in the construction of a cell of very reduced dimensions, required by the short focal distance, prevented the employ-

* Sulphuric ether was chosen as controlling substance, because it could be easily removed by evaporation, accelerated with the air-pump.

ment of one. Its height should have been less than 0·5 centim., and a cell which ought to resist strong pressure was not realisable in this case.

I will now adduce the results of some determinations concerning the thickness of a plane parallel glass plate, made with the microscope, and also to verify the precision with a spherometer allowing to read to a thousandth of a millimetre.

Microscope. (Magnifying power = 30.)		Spherometer.
1·11 millims.		1·0745 millims.
1·06		1·0750
1·12		1·0745
1·08		———
1·06		3·2240
1·07	Mean......	1·0746
1·06		
———		
7·56		
Mean.... 1·08	....	1·08
		———
	Difference........	0·0054 millim.

Calculating the probable error, its value is found for each single determination as $\pm 0·016$, and for the result of a whole series as $\pm 0·006$.

Applying a much higher magnifying power, I could focus on the very clearly defined strips on the scales of Lepidoptera wings; the results were still more satisfactory. As proof, I present the determination of the height of a glass cell, consisting of a short cylindrical tube, having plane parallel plates on both ends.

Microscope. (Magnifying power = 140.)		Spherometer.
5·40 millims.		5·413 millims.
5·42		5·429
5·44		5·413
5·44		5·427
5·43		5·426
5·43		5·432
5·42		———
5·42		32·540
5·40	Mean......	5·423
5·44		
———		
54·24		
Mean.... 5·424	....	5·424
		———
	Difference......	0·001

The calculated probable error of the result amounts to $\pm 0{\cdot}003$.

I will now adduce a complete series of observations taken from preliminary experiments, which I made to become familiar with the microscopical method. They serve also as an elucidation of the manner in which the results recorded into the next chapter were obtained. The following example bears on the determination of the index of refraction of sulphuric ether, and was effected with the glass cell mentioned above, and with a magnifying power of 140.

Determination of the Height D.

Focussing on the upper glass disk............	29·00	24·34	20·25	17·63	14·99	11·53
Focussing on the inferior glass disk............	23·60	18·92	14·81	12·19	9·56	6·12
	5·40	5·42	5·44	5·44	5·43	5·41

Mean .... D=5·42 millims.

Determination of the Correction for the Glass Effect.

Focussing through the cell	3·19	4·67	6·03	8·79	8·67	10·05
Focussing without the cell	2·39	3·86	5·24	8·01	7·86	9·26
	0·80	0·81	0·79	0·78	0·81	0·79

Mean .... 0·80 millim.

Determination of the Index n.

Focussing through the glass cell and liquid ..	22·35	19·00	15·73	9·88	14·36	16·83
Correction for glass.....	0·80	0·80	0·80	0·80	0·80	0·80
	21·55	18·20	14·93	9·08	13·56	16·03
Focussing directly.......	20·13	16·81	13·53	7·64	12·10	14·59
	1·42	1·39	1·40	1·44	1·46	1·44

Mean .... $d=1{\cdot}43$.

$$n=\frac{D}{D-d}=\frac{5{\cdot}42}{5{\cdot}42-1{\cdot}43}=1{\cdot}358.$$

Temperature=13°.

The light employed for the determination was that from a Bunsen's gas-burner, the flame containing some natrium chloride; the value, therefore, refers to the D line. If it is compared to the result obtained with other methods, for instance, that given by Lorenz*, as 1·3599 at 8° and 1·3521 at 21°, leading by interpolation to 1·3569 at about 13°, it is obvious that the concordance is very satisfactory, and

* The values are taken from the recently published "Physikalische und Chemische Tabelle" of Landolt and Bornstein. The temperatures given in this paper always refer to the centesimal scale.

it may be concluded that when some precautions are taken the microscopical method may give reliable results. Of course, a high magnifying power was used in the fore-mentioned experiment that could not be applied when dealing with liquefied gases, still good results are possible even with a magnifying power of 30, as is evident from the following table containing observations on sulphuric ether and water taken in cells of different heights.

Liquid.	D.	*d*.	*n*.	Temp.	Correct value.
	millims.	millims.			
Sulphuric ether ..	16·03	4·23	1·358	14°	1·3594 at 15° and for D line.
	15·77	4·11	1·354	17	
	5·48	1·44	1·356	14	
Water	5·48	1·87	1·333	14	1·3333
	7·74	1·93	1·332	16·5	1·33323
	15·75	3·95	1·335	17·5	1·33309

The determinations from the correct values are within the limits of the probable error, and I must observe that even with very accurate methods, attaining four decimals, the results given by different experimenters vary in the third decimal, for instance, with sulphuric ether at 15°, Gladstone and Dale found the index (D line) 1·3566; Kundt, however, 1·3594.

IV. *Experiments on the Refraction of Liquefied Gases.*

I will now proceed to give the results of the experiments bearing on several liquefied gases, and made as described above. Therefore I think it sufficient, after the foregoing explanations, to give for each gas the final results for the values of D, *d*, and *n*, these having the same signification as in the former chapter. Yet I must add that during a space of four years I repeated at different times the determinations for each gas with different vessels; in this way the number of single observations has amounted to some thousands, consisting of series of six or eight terms. I commonly employed as source of light the flame of a Bunsen gas-burner, containing a piece of woven asbestos, moistened with a solution of natrium chloride, and producing an excellent monochromatic light. Sometimes I also experimented with ordinary daylight; this was necessitated by some calculations, as will be seen in the next chapter.

1. *Sulphurous Acid* (SO_2).

The gas was prepared in the usual way, and liquefied in a tube connected with a parallel-sided glass cell by means of a freeting mixture.

The observations led to $D=15 \cdot 54$ millims., with the natrium light $d=4 \cdot 03$ millims., $n=1 \cdot 350$ at 15°, with daylight $n=1 \cdot 357$ at 13°.

The index of refraction of the easily liquefied gas was already determined by Ketteler in 1865 with a goniometer, and he found the value of n for the D line $=1 \cdot 3384$ at 24°; more recently Bichat* observed $n=1 \cdot 344$ at 18°·5. It may be deduced from these quantities that this quantity changes nearly 0·001 for one degree of change in the temperature; if, therefore, a quantity of 0·003 is subtracted from the value obtained in my experiments, there results 1·347, approaching nearly to the amount 1·344, resulting from an exact determination, and demonstrating again what may be expected from the microscopical method in such cases, and how far it is reliable.

2. *Cyanogen* (C_2N_2).

The gas was prepared from very pure cyanide of mercury strongly dried before, and the liquefaction effected in a bent tube with glass cell provided.

The observations led to $D=16 \cdot 03$ millims., with the natrium light $d=3 \cdot 93$ millims., $n=1 \cdot 325$ at 18°, with daylight $n=1 \cdot 327$ at the same temperature.

In 1826 already D. Brewster† published the amount of the index as 1·316, yet without indicating the method by which it was found, nor the temperature to which it refers; no comparison thus is possible.

3. *Hydrocyanic Acid* (CNH).

The gas was prepared by decomposing the cyanide of mercury by well-dried sulphuretted hydrogen (it had been led through anhydrous phosphoric acid), and condensing with a freezing mixture in the glass cell. It was thus obtained extremely pure, as it showed no alteration after a long space of time.

The observations led to $D=15 \cdot 26$ millims., with the natrium light $d=3 \cdot 09$ millims., $n=1 \cdot 254$ at 19°, with daylight $n=1 \cdot 264$ at 19°.

Determinations of the index were already made by Bussy and Buignet‡ with the prismatic method, giving 1·263 at 17° for the D line. Earlier still, in 1839, Cooper also obtained, in the same way, for daylight, $n=1 \cdot 275$.

4. *Nitrous Oxide* (N_2O).

I could not succeed in preparing the gas obtained by the decomposition of ammonia nitrate in liquefied condition in glass tubes, as

* Bichat, "Journal de Physique," t. ix, 1880, p. 279.

† "Transactions of the Royal Society of Edinburgh," vol. x, p. 407. In this paper there is only said on this subject that Dr. Turner prepared the liquid cyanogen by compression.

‡ Bussy and Buignet, "Annales de Chimie et de Physique" (4), t. iii.

was done by Faraday, as these always exploded. The compression pump of Cailletet, therefore, was extremely useful in this case, and I applied it with its glass tube modified as described in the former chapter. Enough liquid was produced to fill the cell at the top completely at a pressure of nearly sixty atmospheres; a series of observations taken in 1879 led to D=16·34 millims., with natrium light $d=2{\cdot}64$ millims., $n=1{\cdot}193$ at 16°, with daylight $n=1{\cdot}196$, also at 16°. Later on, in 1883, when controlling my former experiments with another glass cell, I found D=15·56 millims., with natrium light $d=2{\cdot}53$ millims., and $n=1{\cdot}194$ at 15°, with daylight $n=1{\cdot}198$. These results are almost identical.

5. *Phosphoretted Hydrogen* (PH_3).

The liquefaction of this gas was also readily effected with the aid of the Cailletet pump. It was prepared from a mixture of phosphonium iodide and a weak solution of potash,* evolving by their reaction a thoroughly pure gas; at a pressure of about thirty atmospheres a perfectly clear liquid filled the glass cells.

The experiments gave D=15·11 millims., with natrium light $d=3{\cdot}64$ millims., $n=1{\cdot}317$, at the temperature 17°·5 with daylight $n=1{\cdot}323$ at 11°.

6. *Sulphuretted Hydrogen* (H_2S).

A series of observations was taken in 1879 with the aid of the compression pump, and the following values on experimenting with the liquefied substance ensued:—D=15·25 millims., with natrium light $d=4{\cdot}19$ millims., $n=1{\cdot}380$ at a temperature of 12°·5.

Later on I repeated the experiments, using the faradaic tube, and liquefied the gas by its own pressure. I obtained D=15·77 millims., $d=4{\cdot}38$ millims., with natrium light $n=1{\cdot}384$ at 18°·5, with daylight $n=1{\cdot}390$ at the same temperature. Instead of leaden disks tinfoil was applied to obtain a perfectly closed vessel, and to prevent any chemical reaction.

7. *Hydrochloric Acid* (HCl).

This gas was evolved in a suitable bent glass tube from a mixture of ammonia chloride and strong sulphuric acid; the very little gas bubbles (evolved under increasing pressure exerted by the gas itself) had to pass, of course, through a long column of this acid, and were thus deprived of any moisture. The liquefied gas did not at all attack the leaden disks used between the glass plates of the cell.

The observations led to D=14·04 millims., with the natrium light $d=2{\cdot}81$ millims., $n=1{\cdot}252$ at 16°·5. Another set of observations

* This method was first proposed by Hofmann, "Ber. Chem. Gesellsch.," 1873, Bd. iv, S. 100.

were made with the gas, liquefied with the Cailletet pump, and gave similar results. I found D=15·10 millims., with natrium light d=3·06 millims., n=1·254 at 10°, with daylight n=1·257, also at 10°.

8. *Hydrobromic Acid* (HBr).

As mercury is attacked either by the gas or liquid I could not avail myself of the Cailletet apparatus in this case, and on this occasion the faradaic tubes served very well. I obtained great quantities of liquefied gas from a mixture of moist potassium bromide and a small quantity of red phosphorus, separated by a paraffine stopper from a column of liquid bromine put in subsequently. The gas evolved passed through granulated zinc (to combine with any bromine vapours) and through calcium chloride, and condensed afterwards into the glass cell as a colourless liquid. Tin disks were also here applied for perfect closure, yet were slightly attacked by the liquefied gas. The determinations led to D=16·08 millims., with the natrium light d=3·95 millims., n=1·325 at 10°, and with daylight n=1·330, also at 15°.

9. *Hydriodic Acid* (HI).

This gas was liquefied in a similar way as above, it being evolved from a moist mixture of red phosphorus iodide and potassium iodide (in a fixed proportion).

The observations led to D=15·68 millims., with natrium light d=4·99 millims., n=1·466 at 16°·5.

10. *Chlorine* (Cl).

Here also only the bent glass tube could serve, as mercury would not have suffered the contact of the gas without combining with it. I prepared it from a mixture of pyrolusite and hydrochloric acid by gently heating and purifying the evolved gas by a long column of pyrolusite and calcium chloride that would absorb some hydrochloric acid and moisture escaped at the same time with the gas, thus a perfectly transparent and bright yellow liquid was obtained.

The observations led to D=15·30 millims., with natrium light d=4·11 millims., n=1·367 at 14°.

11. *Carbonic Acid* (CO_2).

I paid peculiar care to obtain as accurate as possible the index of refraction of this liquid substance that has been studied in various ways. I soon became aware in preliminary experiments that the temperature had more influence than in the other investigations, as might be expected from the circumstance that the critical temperature of liquid carbonic acid is not very far above the surrounding temperature. I preferred therefore the liquefaction in a bent tube by its own

compression to the Cailletet apparatus, because the heat generated in becoming liquid lasts longer than with the other method, where it was always kept ready in liquid state. Another disturbing influence is exerted by some residual air in contact with liquid carbonic acid; discordant results led me to the fact that near to the critical temperature it becomes a very good solvent to air, and the index is then much lowered. The gas was evolved in the tube from a mixture of ammonia bicarbonate and strong nitric acid; and had to pass through calcium chloride before entering into the glass cell. I expelled the air as far as possible with an air-pump and allowing a little reaction between the ingredients a certain quantity of carbonic acid gas was liberated, which, pouring out from the capillary tube at the end of the cell, filled remaining interstices and expelled remanent air, while the tube was sealed.

From numerous series of observations obtained during the last years, I communicate the following numbers taken with different tubes:—

	D millims.	*d* millims.	*n*.	Temp.
I	15·98	2·60	1·196	15°
II	15·60	2·51	1·192	15·5
III	16·05	2·52	1·186	18·5

I has been taken with daylight, the two other observations with the natrium light.

To illustrate the effect of the residual air, I can present the following experiments:—

	I. millims.	II. millims.	Temp.
D	16·01	15·98	
n	1·193	1·199	12·5°
	1·175	1·184	17·5
	1·160	1·173	24°

It is evident that tube I having some air included, and II being nearly free from air, the difference between the values at the same temperature is increasing as this becomes higher (the difference is greater than the probable error). It also appears in this table how the index is very sensibly affected by changes of temperature; this influence is already marked in the second decimal, and the more as the critical temperature is approached.*

* The influence of the temperature on the index of refraction of liquefied carbonic acid, and the changes occurring near and above the critical temperature, will probably form the subject of my next communication.

12. *Ammonia* (H_3N).

I prepared this gas in its liquid condition in the most convenient way by filling a tube with strongly dried granulated calcium chloride; whilst a current of pure ammonia gas was passing through it for some time, the gas became copiously absorbed, and the filled tube served afterwards, by means of a moderate heat, to deliver a large quantity of gas in the glass cell, connected with the tube, which by its own compression became liquid.

The experiments gave D=15·78 millims., and with the natrium light d=3·87 millims., n=1·325 at the temperature of 16°·5, and with daylight n=1·331 at 16°·5. I turned my attention to some amine compounds, which are in chemical properties similar to ammonia, and are prepared from it by substitution.

Among them only methylamine requires liquefaction, as it boils at —4°, and the liquefaction is easily effected by means of a freezing mixture, which is also desirable, when working with dimethylamine and trimethylamine, having boiling points respectively at 9° and 9°·3. I ordered these compounds from the renowned chemical workshops of Kahlbaum at Berlin, whence I received them in sealed glass tubes, and they were distilled in proper bent tubes connected as usual with the parallel-sided glass cells.

I obtained with the natrium light—

	n.	Temp.
Methylamine, CH_5N	1·342	17°·5
Dimethylamine, C_2HN....	1·350	17
Trimethylamine, C_3H_9N ..	1·353	16

The determination of the index for other amines presents no peculiar interest, as their boiling point is high enough to admit the ordinary methods of observation.

13. *Ethylene* (C_2H_4).

The history of the liquefaction of this gas is somewhat curious. It was already liquefied in 1845 by Faraday on application of both increased pressure and low temperatures (42 atmospheres at —1°), but as the notion of critical temperature was then not fully understood, it remained unobserved for this gas, though it is very easily obtained. In 1880 Amagat* published an experimental verification of Boyle's law for this gas, and my compatriot, Professor v. d. Waals of Amsterdam, deduced on theoretical grounds from the results obtained the critical temperature of liquid ethylene, and had it

* Amagat, "Sur la Compressibilité des Gas sous Fortes Pressions," "Comptes Rendus," 1880, t. 91.

confirmed actually by experiment as 9°·2. In 1882 Cailletet* constructed a special apparatus to prepare liquid ethylene in great quantities, and pointed it out as a means of obtaining a much lower degree of cold than was possible before, and this opened the way for the liquefaction of oxygen and hydrogen.

For my purpose I prepared readily a sufficient quantity of the liquefied gas with the ordinary compression pump of Cailletet, taking care to use a very pure gas obtained in the usual way. I collected the liquid in a tube divided in calibrated parts, and could thus determine the specific gravity at different temperatures after the method described by Ansdell,† and applied by him to liquid hydrochloric acid and acetylene. My experiments gave at 8° 0·335, at 6°, 0·361, at 3°, 0·386. For acetylene Ansdell observes that its specific gravity being 0·450 at 0°, it is the lightest fluid substance known; it is surpassed in this regard by liquid ethylene, that has now to be considered as such.

A second tube provided at one end with the parallel-sided vessel already described, at the other end with a specially constructed iron connecting piece,‡ and stopcock cemented on the tube, served for observation of the index of refraction. After the column of mercury in the pump had driven the liquefied gas into the vessel, the stopcock was firmly screwed down, and now the tube with its contents could be removed from the apparatus, and was always ready for observation. Determinations of the index of liquid ethylene could only be made of course when the temperature of the surrounding atmosphere was below 9°·2; I had them in free air, but the winter being very mild this year, I had no opportunity to observe below 5°. The effect of a slight variation of temperature on the refraction is much more marked than with carbonic acid, as was to be expected, and renders an exact result rather difficult. I obtained under satisfactory conditions $n=1{\cdot}180$ for daylight at 6°.

V. *Conclusions.*

Some investigations of a mathematical character have been published in later years that treated of the relation of the refractive power of the same substance considered in different states. The theoretical

* Cailletet: "Comptes Rendus," 1882, t. 94. He mentions on p. 1224 as the critical temperature 13°, and Sarrau (p. 846) nearly 8° or 9°. I found the same value as given by v. d. Waals.

† Ansdell: "Proc. Roy. Soc.," vol. 29, 1879, p. 221.

‡ Such a connecting piece is a most useful appendix to the compression pump, and is constructed also by Ducretet, at Paris. It is very accurately wrought. I kept for more than ten days the liquid ethylene at a pressure from sixty to seventy atmospheres without perceptible leakage, in a tube having the stopcock turned down.

speculations of Professor Lorenz at Copenhagen,* and of Professor Lorentz of the Leyden University have introduced the term the "constant of refraction," expressed by $C=(n^2-1)/(n^2+2)d$, which is independent of dispersion and temperature, and does not vary whether the substance is solid, liquid, or gaseous; n and d represent the index of refraction and the specific gravity. Many years ago very extensive and important observations of Gladstone and Dale, Landolt, Wüllner, were applied to the empirically established relation $C=(n-1)/d$, and they pointed out, that though wanting any theoretical basis, yet it agrees with the results of experiment and fulfils to some extent the conditions of the former expression. Especially has this relation been used in chemistry for comparing the molecular refractive powers of organic compounds, and has again been applied in the extensive and important researches of Brühl.† Far less are the attempts made to test both expressions for the different states of substances, and to ascertain which of them agrees the best with the experimental results. The papers of Lorenz and of Prytz‡ afford together seventeen compounds, nearly all belonging to organic chemistry, and examined as fluids at 10° and 20° and compared to the vapours at 100°. The method used for determining the refraction allowed an exactitude to the fifth and sixth decimal.

I adduce from the paper of Lorenz some results in the following table:—

Substances.	$(n^2-1)/(n^2+2)d$.			Diff.	$(n-1)/d$.		Diff.
	Fluid.		Vapour.		Fluid.	Vapour.	
	10°	20°	100°		10°	100°	
Water, H_2O	0·2062	0·2061	0·2068	7	0·3337	0·3093	56
Sulphuric ether, $C_4H_{10}O$	0·3026	0·3029	0·3068	39	0·4897	0·4602	295
Ethyl alcohol, C_2H_6O........	0·2804	0·2807	0·2825	18	0·4543	0·4206	337
Acetate of ethyl, $C_4H_8O_2$	0·2547	0·2549	0·2683	134	0·4097	0·3821	276
Carbon bisulphide, CS_2	0·2805	0·2809	0·2898	89	0·4899	0·4208	691

* Lorenz deduced this expression from the theory of undulations in a paper published in the Danish language in 1869. A translation appeared in 1881 in the "Wiedemann Annalen," 11, p. 70. Professor Lorentz came to the same relation from the electro-magnetic theory of light, and his investigation was translated in the same periodical in 1880, Bd. 9, S. 641.

† Dr. T. W. Brühl, in "Liebig. Annalen d. Chemie," Bd. 200, 1879.

‡ Lorenz, *loc. cit.*, and Prytz, "Wiedemann. Annalen," Bd. 11, S. 204. Besides these papers, I know of no others bearing on this subject.

To these results I add the following remarks.

1. The values are calculated from the indices determined with the natrium flame, and not corrected for dispersion. Lorenz observes that the deduction of the index of refraction for undulations of infinite length is not possible with the same exactitude as is attained by direct observations with visible light, and prefers, therefore, to apply the experimental results in the formula.

2. As Lorenz has determined by experiment for each substance in its fluid and gaseous condition the amount of dispersion, and this remains nearly the same in both cases, he concludes that this justifies the use made of the expression for undulations of definite length, though it was deduced from those of infinite length.

3. The recorded calculations point out that, though a great change in specific gravity occurs when a substance is passing from one condition into another, yet the relation between the refraction and the density approaches closely to a constant value, the greatest difference amounting to 5 per cent. with the acetate of ethyl. It also appears that the expression established on theoretical grounds answers far better to the experiments than the empirical one, as is evident from the three latter columns in the tables.

I was induced by these considerations to try how far my own experiments would support these results, though less concordance is to be expected, as the exactitude is already impaired at the third decimal of the observed value.

The next table contains my determinations put together with other quantities belonging to the same substance and required for calculation by both expressions. The indices of refraction of the gases are partly taken from the paper of Dulong* and others, occasionally from more recent and accurate researches made by Ketteler, Lorenz, and Croullebois. The densities of H_3N, CO_2, SO_2, N_2O in liquid condition have been accurately determined by Andréef, that of liquid HCl by Ansdell.‡ As in Dulong's researches no special light is mentioned, it must be assumed that daylight was used in determining the refraction; I therefore employed my own corresponding values, taken with common light, for comparison, inasmuch as no recent experiments on gases with natrium light have been published.

From the results in the table, it is again evident that, though no

* Dulong: "Recherches sur les Pouvoirs Réfringents des Fluides Élastiques," 1825. Croullebois: "Annales de Chimie et de Physique," 1870, t. 20.

† Andréef: "Annalen der Chimie u. Pharmacie," 1859, p. 1.

‡ Ansdell: "Proc. Roy. Soc.," vol. 30, p. 221. He applied the compression apparatus of Cailletet, and I followed this very useful method with phosphoretted hydrogen, sulphuretted hydrogen, and ethylene. The specific weights of the other liquefied substances are taken from the "Physik. u. Chem. Tabelle" of Bornstein u. Landolt.

Substances.	Liquid.		Temp.	Gas.		$\frac{n-1}{d}$.		Diff.	$\frac{n^2-1}{n^2+2} \times \frac{1}{d}$.		Diff.
	Density.	Index.		Density.	Index.	Liquid.	Gas.		Liquid.	Gas.	
Sulphurous acid, SO_2......	1·359	1·351	15°	2·234	1·000686	0·252	0·236	16	0·153	0·157	4
Cyanogen, C_2N_2..........	0·866	1·327	18	1·806	1·000822	0·378	0·350	28	0·234	0·233	1
Hydrocyanic acid, CNH ..	0·697	1·264	19	0·944	1·000451	0·379	0·368	11	0 238	0·246	8
Nitrous oxide, N_2O	0·870	1·204	15	1·520	1·000503	0·235	0·255	20	0·150	0·170	20
Carbonic acid, CO_2	0·863	1·196	15	1·529	1·000440	0·227	0·221	6	0·145	0·147	2
Hydrochloric acid, HCl ..	0·854	1·257	10·5	1·247	1·000449	0·300	0·277	23	0·190	0·185	5
Chlorine, Cl	1·33	1·367	14	2·47	1·000772	0·270	0·240	30	0·169	0·160	9
Ammonia, H_3N..........	0·616	1·325	16·5	0·586	1·000373	0·528	0·490	38	0·327	0·327	0
Ethylene, C_2H_4..........	0·361	1·180	6	0·978	1·000669	0·498	0·526	28	0·321	0·350	29
Phosphor. hydrog., PH_3 ..	0·622	1·323	18	1·214	1·000789	0·519	0·500	19	0·322	0·333	11
Sulphur. hydrog., H_2S....	0·91	1·390	18·5	1·191	1·000639	0·429	0·413	16	0·262	0·275	13

Remarks.—The index of SO_2 and C_2N_2 refers to natrium light, and are taken from the determinations of Ketteler (given in the "Physik. u. Chem. Tabelle," von Landolt u. Bornstein, p. 204).

The index of CO_2 refers to daylight, and is taken from the paper of Croullebois ("Annales de Chim. et de Phys., 1870," t. 20), also that of C_2H_4 and H_2S.

The index of H_3N refers to natrium light, and is taken from the paper of Lorenz ("Wiedem. Annalen," 11, p. 103).

perfect concordance between the theoretical relation and the experimental values is necessary, yet the n^2-expression can be adapted in much higher degree to the observations than the empirically established n-expression; and the difference between the calculated and observed quantity is in most cases no more than that existing also in the very accurate experiments of Lorenz, given in the table on p. 358. With the latter expression the value of the proportion is generally decreasing when the substance becomes gaseous; with the former it is increasing; yet there are some exceptions.* Especially with nitrous oxide, there is a greater difference than with others, and I know of no peculiar cause for it, as I repeatedly examined the index. I must observe that also among the substances given in the paper of Lorenz, some of them may be remarked (for instance, the acetate of ethyl) as presenting a far greater difference than others. The ethylene, also, is distinguished among the other substances by a far less concordance, but I may perhaps account for it by the very conditions of the experiment, as this had to be conducted very near to the critical temperature of the liquefied substance. I must add, however, that if, with the value of the proportion given in the table for the liquid ethylene, the equivalent of refraction is calculated, the result is most satisfactory, corresponding to that deduced from refraction equivalents of the composing elements, added together.† Accordingly we have—

$$\text{Refr. equiv. } C_2H_4 = 0{\cdot}321 \times 28 = 8{\cdot}99,$$
$$\text{Refr. equiv. } 2C + \text{Refr. equiv. } 4H = 4{\cdot}86 + 4{\cdot}08 = 8{\cdot}94.$$

Of course such verifications may be made also for the other substances of the former table, and calculated for their liquid condition, but as these may be deduced as well from the gaseous state, and generally with more accuracy, I think there is no peculiar interest in adding the results.‡

* In the experiments of Lorenz this value is for the examined compounds always increasing when they become gaseous; in those of Prytz it is for some increasing, for others decreasing.

† I employed in this calculation the expression $M(n^2-1)/(n^2+2)d$, in which M represents the molecular weight of the compound, and the refraction equivalents of C(=2·43), and of H(=1·02) were taken from the paper of Landolt ("Sitzungsber. Preuss. Acad. v. Wissensch.," 1882, I, p. 43). If the other expression, $M(n-1)/d$, and the corresponding values, C=4·86 and H=1·29, are admitted, the concordance is much less, as we obtain 13·94 (observed) and 14·88 (calculated). Professor Gladstone remarks that as the carbon atoms in this compound, according to current theories of chemical constitution, are to be considered as double-linked, the calculated value is much higher (10·53 or 16·88); the difference from the experimental value is too great to be accounted for by errors of observation.

‡ Landolt, in the paper alluded to above, made such verifications with both expressions applied on nearly fifty organic compounds, and concluded that the n^2 formula gives more accurate results than the other when applied to the index of a

The hydrobromic and the hydriodic acid do not figure in the table, as their index of refraction in the gaseous state has not been ascertained at present. As I examined, however, the index of refraction of liquid bromine* and of liquid chlorine, and as the equivalents of refraction of their combinations with hydrogen may be calculated from my experiments with the liquefied acids, I add here the results, as they can help to elucidate a question suggested by Professor Gladstone, in his former investigation on the refraction equivalents of the elements,† and relating to that of hydrogen especially.

Refraction Equivalent of the Halogens and of their Combinations with Hydrogen.

Substances.	$M(n-1)/d$.		Diff.	$M(n^2-1)/(n^2+2)d$.		Diff.	Remarks.
	Observed.	Calculated.		Observed.	Calculated.		
HCl......	10·9	10·8	0·1	6·9	6·9	0·0	
Cl........	9·6	9·5	0·1	6·0	5·9	0·1	
HBr......	16·2	16·3	0·1	10·0	9·7	0·3	
Br	15·3	15·3	0·0	8·79	8·61	0·2	The refr. eqv. of Br is calculated from $C_2H_4Br_2$.
HI	26·3	26·2	0·1	15·5	15·2	0·3	
I	..	24·9	..	..	14·3	..	The equivalent of I is not directly observed, but calculated from C_2H_5I.

The columns headed with the term "calculated" contain the values‡ that are obtained by addition of the refractive equivalents of the combined atoms; for H and Cl they are given in the paper of Landolt (already alluded to), for both expressions; for Br and I they were calculated by myself. Next to this column are placed the corresponding values of the substances, deduced directly from the

substance compared in liquid and gaseous condition, and that both were only approximative.

* The determination of the index of liquid bromine will be described in the next chapter, and it was ascertained to be 1·571.

† Gladstone: "Proc. Roy. Soc.," vol. 16, p. 443.

‡ I calculated the value of iodine for the n^2 formula with the value given in the "Physik. u. Chem. Tab." of Bornstein u. Landolt as the index for the D line 1·51307, and the density, at 20°, 1·9305.

index of refraction and density* (experimentally determined), and the atomic weight. It is obvious that by subtraction of the proper experimental values we may obtain the mean refraction equivalent of hydrogen, thus determined from observations on the liquid acids themselves, and not in solution. We have for both expressions—

	$M \times (n-1)/d$.	$M \times (n^2-1)/(n^2+2)d$.
HCl—Cl	10·9 − 9·6=1·3	6·9− 6 =0·9
HBr—Br	16·2 −15·3=0·9	10·0− 8·79=1·21
HI—I	26·3 −24·9=1·4	15·5—14·3 =1·2
Mean value of H	1·2	1·10
Calculated value from other substances	1·3	1·04

The conclusion is that the concordance between the value deduced from my experiments and that obtained from other observations is tolerably satisfactory,† and the remark that the equivalent of refraction of hydrogen in an inorganic combination, and especially in an inorganic acid,‡ amounts to 3·5, is not applicable to liquefied hydrochloric acid, nor hydrobromic and hydriodic acid. They were till now only examined in their solutions in water, and it has already been pointed out by Landolt§ and others that the dissolving medium does not exert the same influence on the index of refraction as on the density; my experiments being free from this influence may be deemed appropriate to fix the value of the atomic refraction of hydrogen in these compounds.

* I had to determine the density of liquid hydrobromic and hydriodic acid, hitherto unknown. I could not well apply the method of the compression apparatus, as the mercury is somewhat attacked by the liquefied gases. I therefore employed the Faraday tubes, with glass cell, described before. and provided at the end with a calibrated capillary tube. The tube with its contents and the liquefied gas was weighed; then, by unscrewing the glass cell a little, a certain quantity of the liquid was allowed to escape. The cell was closed and the whole tube weighed again. The diminution in weight observed gave that of the escaped liquefied gas, and its volume was before noted in the capillary part of the tube. I have found the specific weight of liquid HBr=1·63 at 10°, that of liquid HI=2·27 at 12°.

† The difference between calculation and experiment in this case may perhaps be accounted for by the difference existing between the values communicated by various observers for the same substance. In the table the refr. equiv. of iodine given by Landolt as 24·87, is used; if I had admitted the value given by Professor Gladstone as 24·5, the refr. equiv. of hydrogen would have amounted to 1·3, according exactly with other observations.

‡ Professor Gladstone, *loc. cit.* I have shown previously that also from an electro-chemical point of view these liquids differ essentially from their aqueous solutions.

§ Bruhl, *loc. cit.*, p. 222.

VI. *Appendix.*

Determination of the Index of Refraction of Liquid Bromine and some Organo-Metallic Compounds.

I add to this paper the results of experiments with some substances that, though they are liquid at ordinary temperature, yet present some difficulties on being examined by the common methods; their index of refraction has also till now not been observed directly.

The practical efficiency of the microscopical method is well illustrated in the case of liquid bromine, which is very opaque even in thin layers. I had a glass cell constructed with parallel sides in a simple way, by cementing together two plane glass disks, one of them consisting of a thin glass cover (as used in microscopical preparations), with a hole in the centre; this forming the proper vessel when it was finally covered with a glass disk, a single drop of liquid bromine being previously put between. Supplying a 140 magnifying power, enough light (daylight) was transmitted through the layer to permit the focussing on scales of Lepidoptera wings. This layer measured 0·33 millim., the apparent displacement 0·12 millim. (as a mean result of repeated observations); from this the index of refraction is calculated as 1·571 at a temperature of 13°.

I examined also some organo-metallic compounds, especially those which are spontaneously ignited on contact with air. I employed doubly bent glass tubes, ending in the parallel-sided vessel and filled previously with carbonic acid gas. Then I distilled into them from another tube the liquid compound prepared in the usual way, a very small quantity being of course sufficient. I add the following results, referring to the natrium light:—

	n.	Temp.
Zinc ethyl	1·485	12·5°
Zinc methyl	1·474	14
Aluminium ethyl ...	1·480	6·5
Aluminium methyl ..	1·432	12

If these values are compared to those of other metallic compounds, as for instance the tetrachloride of tin ($SnCl_4$), its index amounting to 1·5225* (for the β line of the spectrum of hydrogen), we may remark that it is not necessary for a substance to be very combustible in order to have a very high index of refraction, an opinion that sometimes has been pronounced.

* Haagen, in "Pogg. Ann." (1867), Bd. 151, S. 122. I may also add analogous compounds, as mercury ethyl (1·5397, D line) and mercury methyl (1·5319, D line).

November 20, 1884.

Mr. J. EVANS, Vice-President and Treasurer, in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Professor Walter Noel Hartley and Professor Wilfrid H. Hudleston were admitted into the Society.

Mr. J. Ball, General Boileau, Sir James Cockle, Dr. Rae, and Mr. G. J. Symons, having been nominated by the Chairman, were by ballot elected Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Observations on the Harmonics of a String struck at one-eighth of its Length." By Alfred James Hipkins (of John Broadwood and Sons). Communicated by Alexander J. Ellis, F.R.S. Received October 1, 1884.

The string observed was a steel pianoforte wire, gauge number $19\frac{1}{2}$, diameter 1·17 mm. = ·07 inch, of exactly 45 inches vibrating length, stretched by a tension of 71 kilogrammes = 156·63 lbs., and forming the note C of 135·2 vibrations, in the second space of the bass staff, in one of Broadwood's concert grand pianofortes accurately adjusted to be struck by the hammer at one-eighth the length of the string from the wrestplank end, or 39·375 inches from the belly-bridge. Actually three such strings, forming the usual trichord of a grand pianoforte accurately tuned in unison, were used to augment the volume of tone. The positions of all the nodes less than 39·4 inches from the belly-bridge for the first 20 harmonics, were previously calculated. All three strings were stopped at the same distance from the belly-bridge with the edge of a piece of felt glued to a piece of wood by Mr. Hartan, the foreman of the tuners, while I struck the note. A considerable weight and steadiness of blow was necessary to excite the harmonic. The sound at first was dull

and unmusical, but immediately afterwards the harmonic corresponding to the node touched, sang out, always clearly enough to be unmistakably recognised, and sometimes, especially where the node corresponded to several harmonics, with a long clear ring, that was made brighter and longer by removing the stopper from the string, which then vibrated in the small loops conditioned by the node touched.

The following are the first 20 harmonics of this C determined theoretically. Against each is placed its number of vibrations, and the name of the nearest note on the equally tempered scale, which was that of the piano used, with the theoretical number of hundredths of an equal semitone which had to be added to, or subtracted from, the pianoforte note, in order to give the true pitch of the harmonic. This list enabled the note heard to be immediately identified, by touching the corresponding notes on the pianoforte. Finally in the last column is given the number of inches from the belly-bridge at which one or more of the nodes of the harmonic would lie theoretically, for all the harmonics actually observed and brought out on the 29th July, 1884, in the presence of Dr. William Huggins, F.R.S., who verified the position of the node by a scale after the harmonic had been produced, and Mr. Alexander J. Ellis, F.R.S., who recorded the results. The examination of the numerous other positions of the nodes of these and other harmonics was omitted for brevity, as those possessing most interest had been already produced, but those obtained on other occasions are inserted in a parenthesis. Except in a few instances considered below, the practical place of the node did not differ from the theoretical by more than ·02 or ·03 inch, within which limits it was difficult to be sure of the measurement.

In calculating out the positions of all the nodes it was found that some lay very close together. Thus a node of the 17th harmonic lay at 5·29 inches from the bridge, between one of the 9th harmonic at 5 inches, and another of the 8th at 5·62, so that these nodes were only ·3 inch apart. All these harmonics were brought out separately, but great care was necessary to hit the precise spot. On the other hand the node of the 2nd harmonic at 22·5 inches lay between one of the 19th at 21·32 inches, or 1·2 inches nearer the bridge, and another also of the 19th at 23·68, or 1·2 farther from the bridge. Hence there was a space of 2·36 inches with only one harmonic node within it. Probably in consequence of this the 2nd harmonic could be brought out by touching the string at a considerable distance on either side of the theoretical place, because apparently the string had no other shape which it could assume. It was determined that the limits within which the 2nd harmonic could be brought out were from 22·1 to 22·95 inches from the bridge, allowing ·85 inch play, but at 22·05 and 23·0 inches from the bridge the harmonic would not speak.

Table of Harmonics.

No. of Harmonic.	No. of vibrations in a second.	Name of the nearest equally tempered note.	Distances in inches of the node from the further (belly) bridge at which the harmonic was brought out.
1	135·2	*c*	
2	270·4	*c′*	22·5.
3	405·6	*g′*+2	15·0 (30·0).
4	540·8	*c″*	(11·25, 33·75).
5	676·0	*e″*−14	9·0 (18·0, 27·0, 36·0).
6	811·2	*g″*+2	(7·5, 37·5.)
7	946·4	*b″*♭−31	6·43 (12·86, 19·29, 38·57).
8	1081·6	*c‴*	5·63, 16·88, 28·13.
9	1216·8	*d‴*+4	5·0 (10·0, 20·0).
10	1352·0	*e‴*−14	4·5 (13·5).
11	1487·2	*f‴*+51	4·09 (20·45, 24·54, 28·63, 36·82).
12	1622·4	*g‴*+2	3·75.
13	1757·6	*a‴*−59	3·46.
14	1892·8	*b‴*♭−31	(3·21, 35·36).
15	2028·0	*b‴*−12	3·0 (24·0, 33·0).
16	2163·2	*c⁗*	2·81, 8·44.
17	2298·4	*d⁗*♭+5	5·29.
18	2433·6	*d⁗*+4	12·5 (2·5.)
19	2568·8	*e⁗*♭−2	4·74.
20	2704·0	*e⁗*−14	

Similarly for the 3rd harmonic which had its theoretical node at 15 inches from the bridge, between a node of the 19th at 14·21 inches and one of the 20th at 15·75 inches, leaving 1·64 inches unoccupied by nodes. Practically the 3rd harmonic spoke from 14·75 to 15·4 inches from the bridge, giving a "play" of ·65 inch, but would not come out at 14·7 or 15·45. These were the only two cases in which the amount of "play" was accurately determined. In each case the harmonic came out brightest and best at the theoretical node. Dr. Huggins said that he had remarked a similar phenomenon on the violin, where he found a "play" of about a quarter of an inch in "stopping" for the octave harmonic. Subsequently I brought out the 23rd harmonic *f⁗*♯ + 28, vib. 3124·6, at 1·96 inches from the bridge. And going nearer, at 1·50 inches from, and until quite up to, the belly-bridge, I got out the dull prime note of the string C, without apparently any partial. The same note was produced also at 1·50 inches from the wrestplank-bridge. So that there is still more play for the prime note itself. The stopping seemed to obliterate all the upper partials, but allowed the string to vibrate as a simple tone in its full length. It is remarkable how many harmonics could be elicited by the means adopted from one length of string, and how

clearly the high harmonics even to the 19th came out. Also it was noticeable that the harmonic could be brought out by touching at any one of its nodes, even at those very far distant from the striking-point, showing how accurately a comparatively stout string of great tension could resolve itself into minute sub-vibrations. Thus the 19th harmonic was quite distinctly brought out, so as to be easily compared with $e''''\flat$, by touching the string at its second node from the bridge, leaving 40·26 inches of string to be agitated by the blow.

The object of the experiment was to determine, if possible, the effect of the striking place on the harmonics quenched. Helmholtz ("Tonempfindungen," fourth edition, p. 133) says that the striking place is from $\frac{1}{7}$ to $\frac{1}{9}$ the length, between which, of course, lies $\frac{1}{8}$, and observes (*ibid.*) that "an essential advantage of this striking place appears to be that the seventh and ninth partial tones disappear, or, at least, become very weak." I do not know of any pianos with the striking place at $\frac{1}{7}$ the length. Harpsichords and spinets, which were set in vibration by quill or leather plectra, had no fixed point for plucking the strings. It was generally from $\frac{1}{2}$ to $\frac{1}{10}$ of the vibrating length. Although it had been observed by Huyghens and the Antwerp harpsichord maker, Jan Couchet, that altering the plucking place altered the quality of tone, giving rise to the "lute stop" of the eighteenth century, no attempt was made to fix a uniform plucking place. On the latest improved spinet, a Hitchcock, of the early part of the eighteenth century, in my possession, the striking distances for the C's vary from $\frac{1}{2}$ to $\frac{1}{7}$. And on the latest improved harpsichord, a Kirkman, of 1773, also in my possession, the striking distances of the C's vary from $\frac{1}{2}$ to $\frac{1}{10}$, and the lute stop from $\frac{1}{9}$ to $\frac{1}{29}$ of the string. The bass or longest strings giving, of course, the shortest striking measures, and the same was true of the early pianofortes, as those of Stein, Mozart's favourite pianoforte maker. The great length of the bass strings as carried out on the single belly-bridge, copied from the harpsichord, made it impossible to equalise the striking place for that part of the scale. It was John Broadwood, in 1788, who first endeavoured to equalise the scale in tension and striking place. Assisted by Signor Cavallo and the then Dr. Gray of the British Museum, he produced the divided belly-bridge which enabled him to reduce the length of the bass strings, and hence gained a uniform striking place. He adopted $\frac{1}{9}$ the vibrating length, allowing much latitude in the treble. C. Kützing ("Das Wissenschaftliche der Fortepiano-Baukunst," Bern, Chur, und Leipzig, 1844, pp. 41–2) says the maximum should be $\frac{1}{8}$, and the minimum $\frac{1}{9}$ the length, but that the latter requires a softer hammer to bring out a pleasant tone, and that $\frac{1}{8}$ is much better. The present head of the house of Broadwood (Mr. Henry Fowler Broadwood) has arrived at the same conclusions, and adopted $\frac{1}{8}$ the vibrating length as the striking place for

his pianofortes. Kützing says that when he was an assistant he had to "equalise" instruments where the striking place was between $\frac{1}{12}$ and $\frac{1}{7}$ the vibrating length, and it is the latter place which Helmholtz has adopted for his table of experiments (*ibid.*, p. 135), in which the 6th harmonic is made very weak, and the 7th disappears altogether.

Now the table I have given shows that though the striking place was adjusted with great accuracy to $\frac{1}{8}$ the vibrating length, not only the 7th and 9th harmonics, but also the 8th and even the 16th, were brought out distinctly. The 7th was particularly strong and clear, and the 9th was very good indeed. The 8th harmonic was not so strong, but it was perfectly clear, and it was got out at three of its nodes. Of course, it has 7 nodes, but of the 4 where it was not brought out, 2 were nodes of the 4th harmonic, and 1 a node of the 2nd harmonic, and the 8th was, of course, absorbed in these, while the remaining node was the striking place itself. Perhaps the reason why the 8th harmonic did not disappear was that the striking surface of the hammer was not a perfectly hard edge, but a yielding surface, so that the blow spread on both sides of the intentional striking place, and thus excited the string at very slight distances from the node itself. But whatever may have been the cause the result was quite distinct, and recognised clearly by Dr. Huggins and Mr. Ellis, as well as myself and Mr. Hartan. So that there is no doubt whatever that striking with a pianoforte hammer at a node does not obliterate the corresponding harmonic.

For the 16th harmonic there are 15 nodes, 1 at the striking place, 1 between the striking place and the wrestplank-bridge, neither of which could be tried, 3 which were also nodes of the 8th harmonic, 2 of the 4th, and 1 of the 2nd. The remaining 7 belong to the 16th harmonic alone, and of these it was thought sufficient to try two, which produced the sound quite clearly. The neighbouring 15th and 17th harmonics also came out well. These are an additional proof that a pianoforte hammer striking at a node does not destroy the harmonic due to that node. Subsequently I had an opportunity of trying the middle c' string of one of Steinway's grand pianofortes. This string was 28·75 inches long, and was struck at 3·2 inches from the wrestplank-bridge, that is, at $\frac{1}{9}$ of its length. I got out the 6th, 7th, 8th, and 9th harmonics just as in Broadwood's piano, the 6th and 7th both beautifully strong; the 8th and 9th weaker, but clear and unmistakable—a further confirmation of the fact that the pianoforte hammer does not obliterate the harmonic at whose node it strikes.

II. "Tonometrical Observations on some existing Non-harmonic Musical Scales." By ALEXANDER J. ELLIS, B.A., F.R.S., assisted by ALFRED J. HIPKINS (of John Broadwood and Sons). Received October 30, 1884.

Musical Scales are said to be Harmonic or Non-harmonic according as they are or are not adapted for playing in harmony.

Most accounts of non-harmonic scales, such as the Greek, Arabic, and Persian, either (1) are derived from native theoreticians, who give the comparative lengths of the strings for the several notes, whence, on the assumption that the numbers of vibrations are inversely proportional to the lengths (which is only approximately correct in practice), the intervals from note to note are inferred; or (2) are attempts to express the effects of the intervals by the European equally tempered scale. The former when reduced, as in Professor J. P. N. Land's "Gamme Arabe," 1884, is the best that can be done without hearing the scales themselves. The latter is utterly delusive and misleading.

Having about 100 tuning-forks, the pitch of each of which has been determined by Scheibler's forks (see "Proc. Roy. Soc.," June, 1880, vol. 30, p. 525), and having had an opportunity of hearing the notes themselves produced on various instruments, and having had the great advantage of being assisted by Mr. A. J. Hipkins's musical ear, which is wonderfully acute to detect and estimate minute differences of pitch, and without which I could have done little,* I have been able, I believe for the first time, to take down the actual pitch of the notes in various existing non-harmonic scales far better than it was possible to do with the siren or the monochord, which are not only difficult to manipulate and to carry about, but at the best are very apt to mislead. Where it was impossible actually to hear the sounds, I carefully measured the comparative vibrating lengths of the strings producing the notes on fretted instruments, whence, with by no means the same certainty, the scales could be inferred. But I have not here noted these measurements or their results, unless I could contrast them with the intervals obtained by measuring the actual pitch of the notes produced on the instruments themselves, as in the cases of India and Japan.

But the mere statement of the numbers of vibrations, or of the vibrating lengths of the strings producing a scale, conveys no musical notion whatever to a musician. He wants to know how many equally

* Throughout this paper, "we" and "us" relate to Mr. Hipkins and myself jointly, and all measurements of numbers of vibrations made by us rest on the judgment of Mr. Hipkins's ear with respect to the position of the note heard between two forks, of which I had previously determined the pitch, or their Octaves.

tempered Semitones, or parts of such Semitones, are contained in the interval, so that he can realise it somewhat, as compared with the notes of a modern piano, which are intended to be tuned in equal temperament.* This transformation is easily effected by the following brief table, premising that for brevity I use *cent* for the hundredth part of an equally tempered Semitone, of which there are twelve to the Octave.

To convert tabular logarithms into cents, and conversely—

Cents.	Logs.	Cents.	Logs.	Cents.	Logs.	Cent.	Logs.
100	·02509	10	·00251	1	·00025	·1	·00003
200	·05017	20	·00502	2	·00050	·2	·00005
300	·07526	30	·00753	3	·00075	·3	·00008
400	·10034	40	·01003	4	·00100	·4	·00010
500	·12543	50	·01254	5	·00125	·5	·00013
600	·15051	60	·01505	6	·00151	·6	·00015
700	·17560	70	·01756	7	·00176	·7	·00018
800	·20069	80	·02007	8	·00201	·8	·00020
900	·22577	90	·02258	9	·00226	·9	·00023
1000	·25086						
1100	·27594						
1200	·30103						

take the logarithm of the interval ratio and seek the next. least in the first column of the table; then the next least to the difference, and so on, taking the cents opposite. Generally it suffices to take to the nearest cent, as that expresses an insensible interval. Thus, if the numbers of vibrations are 440 and 528, the difference of their logs. is ·07918; the next least in the first column, ·07526, gives 300 cts., with remainder, ·00392; the next least to which in the second column, ·00251, gives 10 cts., and remainder, ·00141; the next least to which in the third column, ·00125, gives 5 cts., and remainder, ·00016, which in the fourth column gives ·6 ct. Hence the interval is 315·6 cts., for which usually 316 cts. is sufficient to write. Now this shows that the interval contains 3 equal Semitones, and 16 hundredths of a Semi-

* The first person to propose the measuring of musical intervals by equal Semitones was, I believe, de Prony, but I have not been able to see his pamphlet; the next was the late Professor de Morgan ("Cam. Phil. Trans.," x, 129), from whom I learned it, and I employed it in the Appendix of my translation of Helmholtz, by the advice of Mr. Bosanquet. Having found that two places of decimals sufficed for most purposes, I was led to take the second place, or hundredth of an equal Semitone as the unit, and I have extensively employed this practice, here for the first time published, with the greatest advantage. In fact, I do not know how I could have expressed the results of the present investigation in any other brief and precise, and at the same time suggestive, method.

tone more. It is therefore the just minor Third, and it is written between the notes that form it, thus: *A* 316 *C*.

It is convenient for comparison with what follows, to have the following just intervals expressed in cents:—

Intervals.	Cents.	Intervals.	Cents.
The Skhisma	2	Just major Third, $\frac{5}{4}$	386
The Comma of Didymus, $\frac{81}{80}$	22	Pythagorean major Third, $\frac{81}{64}$	408
The Pythagorean Comma ..	24	Grave Fourth, $\frac{320}{243}$	476
The Septimal Comma, $\frac{64}{63}$...	27	Just Fourth, $\frac{4}{3}$............	498
Quartertone	50	Septimal Fifth, $\frac{7}{5}$	583
Small Semitone, $\frac{25}{24}$	70	Tritone, $\frac{45}{32}$	590
Pythagorean Limma, $\frac{256}{243}$...	90	Grave Fifth, $\frac{40}{27}$...........	680
Small Limma, $\frac{135}{128}$.........	92	Just Fifth, $\frac{3}{2}$....	702
Diatonic Semitone, $\frac{16}{15}$......	112	Acute Fifth, $\frac{243}{160}$..........	724
Pythagorean Apotome	114	Just minor Sixth, $\frac{8}{5}$	814
Great Limma, $\frac{27}{25}$..........	134	Just major Sixth, $\frac{5}{3}$........	884
The Trumpet $\frac{3}{4}$ Tone, $\frac{12}{11}$ ...	151	Pythagorean major Sixth, $\frac{27}{16}$	906
The minor Second, $\frac{10}{9}$......	182	Natural minor Seventh, $\frac{7}{4}$ ..	969
The major Second, $\frac{9}{8}$.......	204	Minor Seventh, $\frac{16}{9}$	996
Septimal minor Third, $\frac{7}{6}$ ...	267	Just major Seventh, $\frac{15}{8}$	1088
Pythagorean minor Third, $\frac{32}{27}$	294	Pythag. major Seventh, $\frac{243}{128}$	1110
Just minor Third, $\frac{6}{5}$.......	316	Octave..................	1200

In each scale I give the measured number of vibrations with, occasionally, the millimetres in the vibrating lengths of string, the cents in the interval from note to note, and the sum of those cents from the lowest note to the note considered. From the latter, considering the lowest note to be *c* in all cases, it is easy to deduce the name of the nearest equally tempered note, and show how many cents must be added to it or subtracted from it to give the note heard, by remembering that—

c	*c*♯ or *d*♭	*d*	*d*♯ or *e*♭	*e*	*f*	*f*♯ or *g*♭	*g*	*g*♯ or *a*♭
=0	100	200	300	400	500	600	700	800

a	*a*♯ or *b*♭	*b*
900	1000	1100

It must be borne in mind that I give the actual intervals heard from or measured on actual instruments, and that these, we may safely say, *never* represent the intervals intended by the tuner, within from 5 to 20 cents either way, on account of the extreme difficulty of precise tuning, especially when the intervals are non-harmonic. European ears are at present satisfied, on our theoretical equally tempered scale, with Fifths too flat, and Fourths too sharp by 2 cts., with major Sevenths too sharp by 12 cts.; major Thirds too sharp by

14 cents, and major Sixths too sharp by 16 cents, while of course the minor Sixths are 14 cts. too flat, and the minor Thirds 16 cts. too flat. That is to say, these would be the errors if the tuning were perfect. The practice, as I have determined by actual measurement, is necessarily far from being restricted to these limits. Hence the results here given have to be compared with many other results from other instruments of the same kind, tuned by different tuners before the intended intervals could be, if they ever can be, satisfactorily determined. In the meantime we know that native ears have actually been satisfied by the intervals here given.

It must also be remembered that as the tones heard were often exceedingly brief (as from wood harmonicons), or very impure, being mixed with inharmonic proper tones (as from metal harmonicons, kettles, gongs, &c.), it was generally impossible to count beats, and often even exceedingly difficult to tell within what pair of forks the note heard really lay, so that there is a possible error of two vibrations occasionally, but, thanks to the acuteness of Mr. Hipkins's ear, it is not probable that the error at any time exceeds one vibration in a second. The number determined is therefore purposely given only to the nearest integer.

I. Arabia and Syria.

The theoretical account of Arabic scales is admirably given in Professor Land's "Gamme Arabe." It there appears that one Zalzal, more than a thousand years ago, being dissatisfied with the ordinary division of the Fourth, as—

C	204	*D*	90	*E*♭	114	*E*	90	*F*
0		204		294		408		498

(where the figures between give the number of cents from note to note, and the figures below give the number of cents from the lowest note), introduced a division, which, carried out to the Octave, amounted to—

C	204	*D*	151	q*E*	143	*F*	204	*G*	151	q*A*	143	*B*♭	204	*C*,
0		204		355		498		702		853		996		1200

where q*E* and q*A* mean about a quarter of a tone *less* than (or before coming to) *E* and *A*, and in the same way *E*q, *A*q would mean a quarter of a tone *beyond E* and *A*. (In musical notes q will become ꟼ, a turned ♭.)

In later periods this was tempered to a division of the Octave into 24 equal Quartertones, as we learn from Eli Smith, an American missionary at Damascus, who translated Meshāqah's treatise in the "Journal of the American Oriental Society," 1849, vol. i, pp. 171–217. The scale therefore becomes—

C	200	*D*	150	q*E*	150	*F*	200	*G*	150	q*A*	150	*B*♭	200	*C*,
0		200		350		500		700		850		1000		1200

although in the Middle Ages a different scale prevailed in Arabia, to which I need not further allude. Now between Zalzal's time and this mediæval alteration the Crusaders brought the Syrian bagpipe to England, and after it had passed out of fashion in England, it became the national instrument of the Highlands of Scotland.

Such an instrument, made by Macdonald, of Edinburgh, and obligingly played to us by its possessor, Mr. Charles Keene, the well-known artist, yielded on examination the following results:—

Highland Bagpipe.

Vib.*	395		441		494		537		587		662		722		790		882
From vib. ..	*g'*	191	*a'*	197	*b'*	144	*c''*	154	*d''*	208	*e''*	150	*f''*	156	*g''*	191	*a''*
Sums from *a'*.	−191		0		197		341		495		703		853		1009		1200
Tempered ..	−200		0		200		350		500		700		850		1000		1200
Notes			*c*		*d*		q*e*		*f*		*g*		q*a*		*b*♭		*c'*

The tempered form, therefore, coincides with the Damascus form of Zalzal's scale, which I did not discover till long afterwards. The theory of this scale is lost, but it is usual to make *g'* to *a'* rather less than a whole tone, while the two drones, an Octave and two Octaves below *a'*, necessitate a pure Fifth, *a'* 702 *e''*. Zalzal divided a Pythagorean minor Third of 294 cents into 151 and 143 cents; the modern instrument divides the just minor Third 316 cents, probably, into 151 and 165 parts. We thus get a possible rationalised form of the bagpipe scale, the first attempted, so far as I know. As usual in bagpipe music, I begin the scale on *a'*. I have calculated the vibration to the same base *a'* 441 vib., for both tempered and rational vibrations, to show how close they are to the observed:—

Rationalisation of the Bagpipe Scale.

Observed vib. .	441		494		537		587		662		722		790		882
Tempered vib..	441		495		540		587		661		721		786		882
Rational vib...	441		496		541		595		662		722		794		882
Notes........	*a'*	204	*b'*	151	*c''*	165	*d''*	182	*e''*	151	*f''*	165	*g''*	182	*a''*
Sums of cents .	0		204		355		520		702		853		1018		1200
Ratios	*a'*	8 : 9	*b'*	11 : 12	*c''*	10 : 11	*d''*	9 : 10	*e''*	11 : 12	*f''*	10 : 11	*g''*	9 : 10	*a''*
Ratio from *a'*..	1		8 : 9		22 : 27		20 : 27		2 : 3		11 : 18		5 : 9		2

* In these tables the line "vib." contains the number of vibrations determined by us. The line "from vib." contains the notes, in this case those usually given in bagpipe music, but generally merely distinguished by Roman numerals, I, II, III, &c., with the interval between them in *cents*. The line "sums" gives the sums of these cents, interval by interval, that is, the interval between each note and the lowest. The line "tempered" shows the nearest intervals on an equally tempered scale of 24 Quartertones in the Octave. The "notes" sometimes added, as those due to taking 0 as *c*, as already explained.

II. India.

There are two distinct kinds of scales in India, those of harmonicons, most probably from hill tribes, and those of the stringed instruments belonging to the conquering race.

Balafong from Patna in the South Kensington Museum, a wooden harmonicon strung over a beautifully carved case, consisting of 25 bars (of which we measured 14) containing 3 Octaves and 3 notes. The Roman numerals II, III, &c., indicate the successive bars, I was not measured.

Vib.	158		176		194		214		233		259		279		320
From vib...........	II	187	III	169	IV	170	V	147	VI	183	VII	129	VIII	237	IX
Sums	0		187		356		526		673		856		985		1222

Vib.	320		355		391		434		484		531		582
From vib..........	IX	180	X	167	XI	181	XII	189	XIII	160	XIV	159	XV
Sums, less 1222..	0		180		347		528		717		877		1036

Observe IV 356 cents, and VII 856 cents, which compare with Zalzal in I. Arabia. All the Octaves were too sharp. The old Indian stringed instrument is the *Vina* with frets $\frac{7}{8}$ to $\frac{9}{8}$ inch high, so that by pressing the string behind the fret the pitch can be greatly altered. These frets are shiftable, but are usually fastened with wax. I measured the vibrating lengths of string of many, but I consider the resulting scales not sufficiently trustworthy for record here. This pressing behind the fret is constantly employed to sharpen the pitch by a quarter or half a Tone. The modern *Sitár*, which has practically superseded the *Vina*, is a very long-necked guitar with movable frets. These frets are set for the *rāg* or *rāgini* (tune, key, or mode) in which the musician is going to play. They are high enough above the finger-board to allow pressure behind to exert a sensible effect, but the ordinary method of raising the pitch is to deflect the string by moving the finger with the string transversely along the fret. As, however, the frets are properly set, this deflection is used only for grace notes at the end, suddenly raising the pitch about a quarter of a Tone and returning it to its former position.

H.H. Rája Rám Pál Singh was kind enough to bring his sitár (which he left with me), and setting it in five different manners to play Indian airs to us. After he had done so I measured the position of the frets, so that I could return them to their places. Afterwards we sounded each note, took its pitch, and determined the scale by my forks. This, I believe, is the first time that this has been done for any Indian instrument. The pitch for the open string was not the same as that used by the Rája, for these measurements were not taken till long afterwards, but the relative pitch remained the same. This string, which was an English pianoforte steel wire, replacing the Indian steel wire which was broken, was too thick, and this interfered somewhat with the setting. As I had calculated the

intervals in cents from the vibrating lengths, I add these also in millimetres to show how unsatisfactory are the results thus obtained. The I, II, &c., number the frets used which, however, begun at about the interval of a Fifth from the open string.

First setting of the Sitár—

Vib. lengths ...	*616*		*554*		*503*		*452*		*417*		*380*		*339*		*316 mm.*
From lengths...	I	184	II	167	III	185	IV	140	V	161	VI	198	VII	122	VIII
Sums	0		184		351		536		676		837		1035		1155
Vib.	393		437		479		535		584		650		731		800
From vib.	I	183	II	159	III	191	IV	152	V	186	VI	203	VII	156	VIII
Sums	0		183		342		533		685		871		1074		1230

Second setting of the Sitár—

Vib. lengths ...	616		554		530		452		417		380		355		316 mm.
From lengths...	I	184	II	77	III	276	IV	140	V	161	VI	118	VII	201	VIII
Sums	0		184		261		537		677		838		956		1157
Vib.	393		437		460		535		584		650		693		800
From vib.	I	183	II	89	III	262	IV	152	V	186	VI	111	VII	249	VIII
Sums	0		183		272		534		686		872		983		1232

Third setting of the Sitár—

Vib lengths ...	616		577		518		452		417		389		352		318 mm.
From lengths...	I	113	II	187	III	236	IV	140	V	120	VI	173	VII	177	VIII
Sums	0		113		300		536		676		796		969		1146
Vib.	393		419		471		535		584		634		707		785
From vib.	I	111	II	203	III	220	IV	152	V	142	VI	189	VII	181	VIII
Sums	0		111		314		534		686		828		1017		1198

Fourth setting of the Sitár—

Vib. lengths ...	612		552		500		466		415		368		337		318 mm.
From lengths...	I	179	II	171	III	122	IV	201	V	208	VI	152	VII	100	VIII
Sums	0		179		350		472		673		881		1033		1133
Vib.	397		439		486		523		594		671		737		786
From vib.	I	174	II	176	III	127	IV	220	V	211	VI	162	VII	111	VIII
Sums	0		174		350		477		697		908		1070		1181

Fifth setting of the Sitár—

Vib. lengths ...	607		574		492		461		408		384		332		312 mm.
From lengths...	I	97	II	267	III	113	IV	212	V	105	VI	252	VII	108	VIII
Sums	0		97		364		477		689		794		1046		1154
Vib.	395		416		488		525		594		620		737		784
From vib.	I	90	II	276	III	127	IV	214	V	74	VI	299	VII	107	VIII
Sums	0		90		366		493		707		781		1080		1187

I would draw attention to the great difference in all cases between the two last intervals, I to VII, and I to VIII, as calculated from the lengths of the strings and the number of vibrations. This arose from the string lying naturally further above the frets for the last notes, and hence the tension being more increased by pressing the string to the fret. Also observe how nearly III approaches to 350 cents in the first, fourth, and fifth settings, and VI to 850 cents in the first, second,

and third settings, taking all from the intervals heard. The Indian system of scales is very complex, and differs much from the European.

III. Singapore.

Mr. Hipkins received a *Balafong* or wood harmonicon direct from Singapore, consisting of 24 bars forming 3 Octaves and 3 notes. We measured the central Octave, beginning at bar 8, as follows:—

Observed vib....	312		344		382		427		470		523		569		626
From vib.	I	169	II	181	III	193	IV	166	V	185	VI	146	VII	165	VIII
Sums	0		169		350		543		709		894		1040		1205
Tempered vib. .	312		340		382		429		467		525		572		624
From vib.	I	150	II	200	III	200	IV	150	V	200	VI	150	VII	150	VIII
Sums	0		150		350		550		700		900		1050		1200

The tempered form is given to show that this is one of the Quarter-tone systems, and the tempered vibrations were calculated to show how near they are to the observed.

IV. Burmah.

The *Patala* or wood harmonicon of 25 small neat bars in the South Kensington Museum, No. 1630—'72, "Engel," p. 16, who gives the scale wrongly. We began at the seventh bar from the end, and took an Octave thus:—

Vib.	300		332		367		408		451		504		551		616
Bars	I	176	II	174	III	183	IV	174	V	192	VI	154	VII	193	VIII
Sums	0		176		350		533		707		899		1053		1246

The Octave is very sharp, and bars 15, 16, 20 were sharp Octaves of II, III, VII, bar 16 being very sharp indeed. Otherwise the Octaves were fair.

A *Balafong*, in South Kensington Museum, with a box decorated with Burmese ornaments, 22 bars, containing 3 Octaves and 1 note. Twelve bars measured from 4th to the 15th. The first 5 formed the end of an Octave.

Vib.	...		...		...		237		258		282		318		353
From vib.	...		...		...		IV	147	V	154	VI	208	VII	181	VIII
Sums	...		...		...		506		653		807		1015		1196
Vib.	353		377		432		485		525		573		641		705
From vib.	VIII	114	IX	236	X	200	XI	137	XII	151	XIII	194	XIV	164	XV
Sums	0		114		350		550		687		838		1032		1196

The sums in the first line have been found by subtraction from that under VIII, which was assumed to be the same as that under XV. The different construction of the corresponding parts of the Octave is thus shown.

The *Keay Wine* in South Kensington Museum consists of 15 kettles or gongs resembling the Javese bonangs, arranged in a circle. III* was

cracked, and its pitch is doubtful, as was also that of V*. II and III*, as the latter stood, were practically identical.

First oct. vib...	303		333		334		377		416		449		506		602
From vib........	I	163	II	5	III*	210	IV	170	V*	132	VI	267	VII	301	VIII
Sums	0		163		168		378		548		680		947		1248
Second oct. vib.	602		622		648		719		796		867		990		1032
From vib.	VIII	57	IX	71	X	180	XI	176	XII	148	XIII	230	XIV	72	XV
Sums	0		57		128		308		484		632		862		934

The kettles were probably all out of tune.

V. Siam.

The *Ranat* in South Kensington Museum is a wood harmonicon with 19 bars, scale wrongly described in "Engel," p. 316. Bar XIII* was of a different kind of wood, and had evidently been inserted as a substitute for the Octave of VI, but was too sharp.

First oct. vib....	323		348		379		433		491		504		585		666
From vib.	VI	129	VII	148	VIII	231	IX	218	X	45	XI	258	XII	225	XIII*
Sums	0		129		277		508		726		771		1029		1254

Second oct. vib.	666		748		794
From vib.	XIII*	201	XIV	103	XV
Sums	0		201		304

The scale is enigmatical.

VI. West Coast of Africa.

This is inserted out of geographical position, because it is a solitary example from Africa, and resembles those immediately preceding in character. A *Balafong* in South Kensington Museum, No. 1080, 1080*a*—'68, "Engel," p. 154, who describes the scale wrongly. We measured nine bars—

Observed vib..	327		357		386		445		497		547		596		654		714
From vib......	VIII	152	IX	135	X	246	XI	191	XII	166	XIII	149	XIV	161	XV	152	XVI
Sums...........	0		152		287		533		724		890		1039		1200		1352
Tempered vib.	327		357		389		449		504		550		600		654		
From vib.......	VIII	150	IX	150	X	250	XI	200	XII	150	XIII	150	XIV	150	XV		
Sums...........	0		150		300		550		750		900		1050		1200		

where the tempering shows that the scale belongs to the system of Quartertones.

VII. Java.

The scales were observed from the instruments of the Javese Gamelang or band, at the Aquarium, in November, 1882, and formed the commencement of these investigations. We were materially assisted by work done on the same instruments (but without determining pitch) by Mr. W. Stephen Mitchell, M.A., of Gonville and Caius College, Cambridge, and by determinations with the monochord of similar instruments in Holland by Professor J. P. N. Land (who

also gave me much information), assisted by Dr. Onnes, both of Leyden. Professor Land also kindly communicated the results of the measurements by Dr. Loman and Dr. Figée, both of Leyden. These measurements of distinct instruments are annexed in a reduced form.

There are two entirely different Javese orchestras which cannot play together. We examined three sets of instruments from each—the *Gambang*, or wooden harmonicon, the *Sáron* and *Slèntem*, or metal bar harmonicons, and the *Bonang*, or set of kettles—while in Leyden a *Gĕndér* (another metal harmonicon) and a different *Sáron* were examined.

The first orchestra played *Salêndro*, the second *Pèlog* scales, both Pentatonic; but, as will be seen, completely different. The first had only five notes in the Octave, the second had seven, but used only five at a time, just as Europeans have twelve, but use only seven at a time. The first has no interval between consecutive notes so small as a major Second, or so large as a minor Third. The second has between two consecutive notes of its seven, approximatively two Semitones, (no Tone), three Three-quartertones, and two minor Thirds. The first is very uniform, the second very diverse in its intervals.

First or Salêndro Scales.

	Obs. vib.				* Out of tune.		† Not recorded.				
Gambang	*268		308		357		411		470		*535
Sáron (E. & H.) ..	272		308		357		411		471		543
Slèntem..........	270		308		357		411		469		540
Mean	270		308		357		411		470		540
From Mean ..*...	I	228	II	256	III	244	IV	232	V	240	I′
Sums	0		228		484		728		960		1200
Gĕndér, lower oct..	I	191	II	251	III	249	IV	261	V	220	I′
Gĕndér, upper oct. .	I	219	II	256	III	261	IV	223	V	288	I′
Sáron (Land)	I	270	II	200	III	266	IV	239	V	243	I′
Sáron (Figée)	I	275	II	210	III†		IV†		V	243	I′
Tempered vib......	270		310		356		409		470		540
From vib.	I	240	II	240	III	240	IV	240	V	240	I′
Sums	0		240		480		720		960		1200

This tempered form seems to have been that aimed at. It is easily tuned when the ear has become accustomed to the flat Fourth of 480 cents. Tune up I 480 III, and III 480 V. Then from the Octave I′ tune down I′—480 IV, and IV—480 II. Observe that the Fourth is flat and the Fifth sharp, and that V is nearly the natural harmonic Seventh of 969 cents. These are also points of distinction from the next set.

Second or Pèlog Scales.

Obs. Vibrations.					* Out of tune.										
Gambang	*283		*311		365		391		416		448		*532		*566
Bonang	278		302		361		390		417		448		526		556
Sàron	279		302		360		387		414		447		524		558
Adopted	279		302		361		389		415		448		526		558
From adopted........	I	137	II	309	III	129	IV	112	V	133	VI	278	VII	102	I′
Sums	0		137		446		575		687		820		1098		1200
							Scales.								
Pèlog	I	446		...	III	129	IV	112	V	411		...	VII	102	I′
Dantsoe	I	137	II	550	...		...	...	V	133	VI	278	VII	102	I′
Bem (E. & H.)	I	137	II	438	...		IV	112	V	411		...	VII	102	I
,, (Loman)	I	147	II	416	...		IV	96	V	429		...	VII	112	I′
Barang (E. & H.)...	I	137	II	438	...		IV	112	V	133	VI	380	...		I′
,, (Loman)...	I	151	II	426	...		IV	111	V	179	VI	333	...		I′
Miring..................	I	446		...	III	129	IV	245	...		VI	278	VII	102	I′
Menjoera..............	I	137	II	309	III	129	IV	523	...		...	...	VII	102	I′
Tempered vib........	279		304		362		395		418		443		527		558
From vib.	I	150	II	300	III	150	IV	100	V	100	VI	300	VII	100	I′
Sums....................	0		150		450		600		700		800		1100		1200

After giving the three sets of vibrations observed I give that adopted, which is the mean of the second and third set, as the Gambang was evidently rather out of tune, and then the scale of all the seven notes answering to the chromatic scale of our pianos. Then follow the names of the scales really used, formed by selecting five notes from these. Pèlog and Dantsoe (pronounce Dutch *oe* as our *oe* in *shoe*) are given only from our own observations. In Bem and Barang, Dr. Loman's observations made with the monochord in 1879 on another set of instruments are added in a reduced form. These four scales are certain. Miring and Menjoera (pronounce Dutch *joe* like the English word *you*) are conjectural restorations from imperfect indications communicated to me by Professor Land. Finally, I have added a rather hazardous tempering, and shown by calculating the vibrations from it, that it does not materially misrepresent the observed. In these scales the Fourth, IV 575 cents, is nearly the tempered Tritone 600 cents, and the Fifth, V 687 cents, is flatter even than the tempered Fifth 700 cents. This is exactly contrary to the Salêndro scale. Yet I observed one of the players selecting the right bar for his scale by holding it up and tapping it with his finger, showing that the pitch was quite familiar to him.

VIII. China.

Without entering upon any discussion on the very vexed question of Chinese music, I confine myself to giving the scales which (by the kind permission of Mr. J. D. Campbell, one of the Commissioners of Chinese customs representing China at the International Health Exhibition this year, and with the assistance of the secretary, Mr. Neumann), we were able to have played to us by the Chinese

musicians attached to that court, in July and August, 1884, at four specially arranged meetings, on their own instruments, together with observations on a duplicate of one of them at the South Kensington Museum, and a set of bells belonging to Mr. Hermann Smith.

1. *Transverse Flute* or *Ti-tsu*, with seven finger holes and an embouchure, open at both ends. Probably in actual playing some of the notes may have been varied by half or quarter covering of the finger-holes. The Heptatonic scale played is given first, and then the notes selected for the more usual Pentatonic scale.

Vib.	240		266		292		311		352		401		454		479
From vib.	I	178	II	161	III	109	IV	214	V	226	VI	215	VII	93	I′
Sums..............	0		178		339		448		662		888		1103		1196
Pentatonic	I	178	II	270	...		IV	214	V	226	VI	308	...		I′

2. *Oboe* or *So-na*, played with a short reed, having seven finger-holes in front and two thumb-holes behind, a loose brass cone of considerable size covered the lower end. Said to be a modern instrument. Sound and intervals resembling the bagpipes.

Vib.................	400		435		475		516		578		640		719		808
From vib.	I	145	II	152	III	143	IV	197	V	176	VI	201	VII	202	I′
Sums	0		145		297		440		637		813		1014		1216
Tempered vib...	400		436		476		519		582		635		713		800
From vib.	I	150	II	150	III	150	IV	200	V	150	VI	200	VII	200	I′
Sums	0		150		300		450		650		800		1000		1200

On this instrument as thus played there was nothing approaching a Fourth of 498 cents, or a Fifth of 702 cents. It must have been modified in playing to work with the flute. Both were orchestral instruments.

3. *Reed Mouth Organs* or *Shêng* (rhymes to *sung*, and often so called), a gourd with its top cut off, and covered with a flat board, in which were inserted 13 pipes, 11 of which had free reeds, which sounded on blowing (or sucking) through the mouth-hole, and stopping a hole in the pipe which the player intended to sound. The lengths of the pipes are ornamental, an internal slot determining the real lengths. The two "dummies" were for holding.

First oct. vib....	450		508		547		600		680		760		820		899
From vib.	I	210	II	128	III	160	IV	217	V	193	VI	132	VII	159	I′
Sums	0		210		338		498		715		908		1040		1199
Second oct. vib.	899		1017		1110		1232								
From vib.........	I′	214	II′	151	III′	182	IV′								
Sums..............	0		214		365		547								
Tempered vib...	450		505		551		601		674		757		825		900
From vib.	I	200	II	150	III	150	IV	200	V	200	VI	150	VII	150	I′
Sums	0		200		350		500		700		900		1050		1200

Here we have a perfect Fourth, IV 498 cents, and a good but sharp Fifth, V 715 cents. But the instrument, if in tune (small free reeds easily fall out of tune), belonged to the Quartertone system.

4. *First Chime of Small Gongs* or *Yan-lo*, a set of 10 small gongs about the size and shape of cheese-plates, arranged with I at the top, II, III, IV in the first row, from left to right behind, where they were struck with a wooden hammer, and then V, VI, VII in the second, and VIII, IX, X in the third row, all hung in a square wooden frame. The Chinese musician played in the order of pitch, omitting IX and I.

Vib.	449		495		555		568		630		663		703		712		830		902
From vib.......	VIII	169	V	198	II	40	IX	179	IV	88	VI	101	X	22	I	265	VII	144	III
Sums...........	0		169		367		407		586		674		775		797		1062		1208
Played	0		169		367		...		586		674		795		...		1062		1208

Here again there is no approach to a Fourth of 498 cents, or a Fifth of 702 cents.

5. *Second Chime of Small Gongs* or *Yan-lo*, in the S. K. Mus., "Engel," p. 193, who describes the scale wrongly. Although the instrument is of the same appearance as the last, the scale was entirely different, and the compass did not reach 750 cents. We seemed to make out three possible scales which are annexed, but we have no means of knowing if they were designed. One extends to a sharp and another to a flat Fifth, whilst the third reaches an exact Fourth. The gongs are numbered as in No. 4.

Vib.	794		818		912		926		1011		1022		1114		1116		1198		1216
From vib.......	I	52	II	188	III	26	IV	152	VI	19	VIII	149	V	3	IX	123	X	26	VII
Sums...........	0		52		240		266		418		437		586		589		712		738
Possible scales																			
To sharp Fifth	I	240		...	III	178		...	VI	171		...	...		IX	123	X		
Sums...........	0				240				418						589		712		
To flat Fifth ...		...	II	188	III	178		...	VI	168		...	V	152		...		...	VII
Sums			0		188				366				534						686
To Fourth......		...		...	III	197		...	...		VIII	152		...	IX	149		...	VII
Sums					0						197				349				498
Tempered					0						200				350				500

The last is therefore like the first tetrachord in the bagpipe scale, dividing the Fourth into a Tone and two Three-quartertones. There are, however, several curious intervals.

VI 19 VIII	nearly a comma of 22 cents.
III 26 IV	nearly $\frac{1}{8}$ of a major Tone of 204 cents.
I 52 II	exactly $\frac{1}{4}$ of a major Tone of 204 cents.
II 188 III and III 178 IV	are both nearly the minor Tone of 182 cents.
I 240 III	is an exact pentatone, or $\frac{1}{5}$ Octave, as in the tempered Javese Salêndro scale.
II 385 VIII	is an excellent major Third of 386 cents.
I 586 V	and I 589 IX are both nearly the Zaïd of 588 cents, on the second string of the Arabic lute.

I 738 VII, the complete compass, is exactly the 49th harmonic reduced to the same Octave, which is of course only a curious coincidence.

6. *Dulcimer* or *Yang-chin*, exactly like the ordinary dulcimer (see figure in Grove's "Dictionary of Music," i, 469), with four wires to each note forming two Octaves, the longer wires passing under the bridge which limits the shorter. It is struck with elastic hammers. The instrument being out of tune was tuned for us by the musician who played No. 7, according to the Chinese names of the scale in Dr. William's *Middle Kingdom*, which are there interpreted as the major scale of *E*♭. If the conjectural just scale be correct, this would be the scale of *B*♭ major, beginning on its second note *C*, and is therefore comparable to the Japanese Ritsusen, which is the scale of *C* major begun on its second *D*.*

Chinese names...	Ho		sz'		i		chang		ché		kung		fan		liu.
Vib.	205		226		240		272		300		340		364		409
From vib.	I	169	II	105	III	217	IV	170	V	217	VI	118	VII	202	I
Sums.	0		169		274		491		661		878		996		1198
Conjectured Just															
Vib.	205		228		243		273		304		342		364		410
From vib.	C	182	D_1	112	$E\flat$	204	F	182	G_1	204	A_1	112	$B\flat$	204	c
Sums	0		182		294		498		680		884		996		1200
Pentatonic form.	C	182	D_1	316	...		F	182	G_1	204	A_1	316	...		c

The tuner had great difficulty in tuning the semitones II 105 III and VI 118 VII, that is, in tuning the notes III and VII. He accomplished the second more easily than the first. The Pentatonic form consists of two disjunct tetrachords, *CF*, *Gc*, each divided into a Tone and a minor Third.

8. *Tambowra* or *Sien-tsu*, a three-stringed guitar with circular body and long neck without frets. The strings were tuned to 239, 266, and 400 vib., making the intervals 185 and 706 cents, meant for 182 the minor tone, between the first and second, and for 702, a Fifth, between the Second and Third, very fairly tuned indeed. The strings were plucked with bone plectrums, attached to the first joint of thumb and forefinger, and projecting like claws. The tone was good and very like a banjo. Only the following pentatonic scale was played to us:—

Vib.	320		357		400		480		536		642
From vib.	I	189	II	197	III	316	IV	191	V	312	I′
Sums	0		189		386		702		893		1200

* In writing tones in Pythagorean intonation formed by a succession of just Fifths or Fourths from *C*, the ordinary letters are kept unchanged; but for just intonation it is necessary to have a series a comma lower. These have a subscript 1, as D_1, so that, in vibrations, $D_1 : D = 80 : 81$. Similarly another series would be a comma sharper, and be written with a superior 1, as $E^1\flat$, so that, in vibrations, $E\flat : E^1\flat = 80 : 81$.

Conjectural Just											
Vib.	320		356		400		480		535		640
From vib.	C	182	D_1	204	E_1	316	G	182	A_1	316	c
Sums..............	0		182		386		702		884		1200
Transformed sums..	498		680		884		0		182		498

This was again so nearly just that I have conjectured a just restoration, $C\,D_1\,E_1\,G\,A_1\,c$: and if this is transformed, by beginning it with G, or by deducting 702 cents from each of the last sums (previously adding 1200 cents where needed), we obtain the scale G 182 A_1 316 C 182 D_1 204 E_1 316 G, in which the intervals are precisely the same as in No. 7.

9. *Balloon Guitar* or *P'i-p'a.*—The body of the guitar was oval. There were four strings, the lowest tuned to 234 vib., and then its Fourth, its Fifth, and its Octave, but we did not test the accuracy of these intervals, which were tuned by the same musician who tuned Nos. 7 and 8. Near the nut were four large, round-backed, semi-elliptical frets, joining each other at bottom. These the player did not use. But on two examples of the S. K. Museum, I conjectured by measuring the strings, that they were intended to give such a tetrachord as—

C	204	D	90	$E\flat$	114	E	90	F
0		204		294		408		498

or their just or tempered forms. There were 12 frets on the body of the instrument. They were high but broad at the top. We did not test each, but merely took down the following pentatonic scale :—

Observed vib..	320		348		392		465		530		638
From vib.	I	145	II	206	III	296	IV	227	V	321	VI
Sums	0		145		351		647		874		1195
Tempered vib..	320		349		392		466		538		640
From vib.	I	150	II	200	III	300	IV	250	V	300	VI
Sums	0		150		350		650		900		1200

The tempered scale agrees well in all notes but V. The scale is so remarkable in every way, though it did not sound amiss, that I suspect the frets to have been inaccurately placed; they were bits of wood roughly glued on.

This completes our observations with the Chinese musicians. I measured also the vibrating lengths of strings in two other P'i-p'as, and also two Moon Guitars or *Yueh-chins* in the S. K. Museum. One of the latter seemed intended for equal temperament of 12 Semitones, and it is the only Chinese instrument which has suggested this to me; the other looked like an attempt to divide the Octave into eight Three-quartertones, and had at any rate eight tones

to the Octave forming nearly those intervals. But as I did not try these with forks I do not record them.

10. *Small Chime of Bells*, belonging to Mr. Hermann Smith. Four small bells of which the largest was 45 mm. in diameter and 13 mm. in height, arranged on a stem passing through them and framed in a lyre-shaped wire.

Vib............	761		912		1004		1156
From vib........	I	313	II	167	III	244	IV
Sums..........	0		313		480		724

The I 313 II is nearly a perfect minor Third of 316 cents. The III and IV give almost precisely the Javese Salêndro observed III 484, and IV 728, so that the interval between them, 244 cents, is almost precisely a Pentatone of 240 cents, or $\frac{1}{5}$ Octave. If indeed II were flatter, the notes of the bells might pass as part of such a scale.

IX. Japan.

In the Educational Section of the International Health Exhibition of 1884 there was a considerable collection of Japanese instruments, but there were no players. The only instruments which we could try therefore were a Shō (the Chinese shêng (see China, 3), but different in the number and pitch and intervals of the notes) and a Biwa, or four-string fretted lute. The Shō we found to be out of tune, as referred to the scale exhibited, and to be impossible to blow satisfactorily. The Biwa I first tried by measuring the lengths of the strings, and afterwards with Mr. Hipkins, by tuning the strings arbitrarily and taking the pitch from each fret. These results I record, because in addition to the examples from India, they show very well that measurements of lengths are only an approximation to the speaking values of the strings, and that the latter vary considerably with the thickness of the strings. This has an important bearing upon the theoretical determination of scales given by the divisions of the string. The results for India were valuable in this respect, but they were not altogether satisfactory, because the string was English and too thick. In the present case we had the genuine Japanese strings.

The *Biwa* is a large and heavy but handsome instrument, well made and finished, and answers exactly to Al Fārābī's lute in Professor Land's "Gamme Arabe," the four strings nearly coinciding at the nut, passing over a semi-circular depression to the large tuning pegs, and spreading out to a convenient distance apart by the bridge, so that the plectrum, made of hard wood, spread out like the head of a halbert, could easily be inserted between the strings, or pass over them in rapid succession for arpeggio chords for which the instrument

seems to be much used in accompaniments, judging from some music written for it in Japan, on the European staff, the original of which I saw. The diameters of the strings, which seemed to be of hard-corded silk, taken by one of Elliott's micrometer gauges, were 1·65, 1·37, 1·06, and 0·88 mm. in diameter respectively. The variations of interval, however, with the thickness of the string appear not to follow any precise law. The frets were high and about 5 mm. wide of the top, made of hard wood. I was very careful to press on the top of the fret, so that the tension of the string might not be increased, and the action should take place from the edge of the fret nearest the bridge. But possibly I may not always have pressed near enough to the edge, so that the string was slightly lengthened and the pitch flattened. Of course nothing like such accuracy would be reached by the player.

Lengths	843		750		709		673		637 mm.
From lengths	I	202	II	97	III	90	IV	95	V
Sums	0		202		299		389		484
Lowest string.									
Vib.	166		189		201		211		223
From vib.	I	225	II	107	III	84	IV	96	V
Sums	0		225		332		416		512
Second lowest string.									
Vib.	167		190		203		214		223
From vib.	I	223	II	115	III	91	IV	71	V
Sums	0		223		338		429		500
Second highest string.									
Vib.	226		253		272		286		301
From vib.	I	195	II	125	III	87	IV	89	V
Sums	0		195		320		407		496
Highest string.									
Vib.	300		339		361		381		401
From vib.	I	212	II	109	III	93	IV	89	V
Sums	0		212		321		414		503
Mean from vib.	I	214	II	114	III	89	IV	86	V
Sums of mean	0		214		328		417		503
Possibly	I	204	II	114	III	90	IV	90	V
Sums	0		204		318		408		498

Hence the division was probably meant for Pythagorean, the last sums giving *C D D♯ E F*, which should have been *C D E♭ E F*, that is, the second Semitone should have been of 114 cents, and the first of 90 cents. Now it appears from the Report of Mr. Isawa, Director of the Institute of Music, Tokio, Japan (founded October, 1878), an English translation of which, prepared at the Institute, was in the Section, that Japanese theory considers its Semitones to be 12 equal

divisions of the Octave, just as in Europe we so consider our 12 Semitones.* Hence these divisions are taken, and are used as—

C	200	*D*	100	*E*♭	100	*E*	100	*F*,
0		200		300		400		500

as they would be played on the pianoforte.

This Report contains an account of the Japanese scale, from which, to complete this notice of Japan, although not tonometrically observed, I may cite the following, where all notes may be provisionally considered as those on the piano.

Classical Scales.

Riosen	*D*	*E*	*F*♯	*G*♯	*A*	*B*	*C*♯	*d*
In descending often	*D*	*E*	*F*♯	*G*	*A*	*B*	*C*♯	*d*
Pentatonic	*D*	*E*	*F*♯		*A*	*B*		*d*
Ritsusen......................	*D*	*E*	*F*	*G*	*A*	*B*	*C*	*d*
Pentatonic	*D*	*E*		*G*	*A*	*B*		*d*

Popular Scales—Heptatonic.

First Heptatonic	*D*	*E*♭	*F*	*G*	*A*	*B*♭	*C*	*d*
Second „	*D*	*E*♭	*F*	*G*	*A*♭	*B*♭	*C*	*d*

Popular Scales—Pentatonic.

Hiradioshi	*G*	*A*	*B*♭	*D*	*E*♭	*G*
Akebono I..................	*G*	*A*	*B*♭	*D*	*E*	*G*
Akebono II.	*A*	*B*♭	*D*	*E*	*F*	*A*
Kumoi I.	*G*	*A*♭	*C*	*D*	*E*♭	*G*
Han-Kumoi	*G*	*A*	*C*	*D*	*E*♭	*G*
Iwato	*G*	*A*♭	*C*	*D*♭	*F*	*G*
Han-Iwato	*G*	*A*♭	*C*	*D*	*F*	*G*

where observe the numerous examples of the most ancient Greek tetrachord of Olympos, consisting of a Semitone followed by a major Third.

* Professor Ayrton, F.R.S., who was present when this paper was read, and who had returned from Japan only a few years ago, made some remarks to which with his permission I will here refer. He said that it was a mistake to suppose the Japanese musical intervals to be like the European. He had examined Japanese instruments when tuned in their different ways by natives, and taken the pitches of the notes by means of a siren, and he had found the intervals very different. My paper in this part merely professes to give Mr. Isawa's theory, without citing his confirmatory experiments, which I did not consider conclusive.—A. J. E.

III. "The Influence of Stress and Strain on the Physical Properties of Matter. Part II. Electrical Conductivity—*continued.* The Alteration of the Electrical Conductivity of Cobalt, Magnesium, Steel, and Platinum-Iridium produced by Longitudinal Traction; Discovery of simple Relations between the 'Critical Points'* of Metals." By HERBERT TOMLINSON, B.A. Communicated by Professor W. GRYLLS ADAMS, M.A., F.R.S. Received October 7, 1884.

(Abstract.)

The effect of temporary longitudinal traction on the electrical resistance of cobalt was determined by a method similar to that already described in a former portion of this memoir,† and it was found that, like nickel, this metal has its resistance *decreased* by moderate temporary stress, in spite of the changes of dimensions which ensue. Whether the decrease of resistance would be changed to increase, as it is with nickel, by a greater amount of stress, has not yet been ascertained, but should this be the case, the magnitude of the stress per unit area which would suffice for the purpose must be much greater with cobalt than with nickel. As with nickel, permanent extension and rolling diminish the effect of temporary longitudinal traction, so that there is a larger decrease of resistance caused by a given stress with annealed than with unannealed cobalt.

Cobalt is remarkable for the extreme persistence with which the same load, when applied again and again, continues to produce permanent increase of resistance, and probably increase of length, but for a moderate amount of permanent extension the increase of resistance is more than accounted for by the permanent increase of length and diminution of section which take place; so that, as with iron and nickel, the *specific* resistance is *decreased* by moderate permanent extension. The permanent decrease of specific resistance per unit for unit permanent increase of length is, for iron, cobalt, and nickel, 0·02, 1·44, and 2·37 respectively; thus the permanent decrease of specific resistance, as well as the temporary decrease of resistance, is greater with nickel than with cobalt.

Since contrary to the expectation arising from the apparent relationship which exists between the "rotational coefficient" of Hall, and the alteration of specific resistance caused by mechanical stress,‡ cobalt behaved like nickel, and not like iron; the effect of longitudinal

* Points at which there is a sudden increase in the ratio of the permanent extension produced by any load and the load itself.

† "Phil. Trans.," Part I, 1883, p. 58.

‡ *Loc. cit.*, pp. 167, 168.

traction on the thermo-electric qualities of cobalt was examined, and it was found that temporary longitudinal traction renders cobalt temporarily *positive* as regards its thermo-electric qualities, to cobalt not under traction, provided there is no magnetic stress acting at the same time on the metal. The results obtained would, therefore, not only be adverse to the above-mentioned hypothesis of the author, but also to Bidwell's theory that the "Hall effect" is due to the joint action of mechanical strain and certain "Peltier effects," were it not that in Hall's experiments the metal is under very considerable magnetic stress, which latter has been proved to alter very appreciably the thermo-electric properties of iron, nickel, and cobalt,* and under these circumstances both the effect of mechanical stress on the specific resistance† and that on the thermo-electric qualities of cobalt may be different to what they are, when there is no magnetic stress acting on the metal.

Longitudinal magnetic stress was found to render cobalt negative thermo-electrical to cobalt not under stress.

The cobalt used was in the form of a strip, and the section, which was fairly uniform throughout the whole length, was 0·06168 square cm.; the length was 58·4 cm., and the mass 29·65 grams.

The value of "Young's modulus" was determined by the method of longitudinal vibrations, the strip being held in the centre, and rubbed along its length with a resined glove.

The effect of longitudinal traction on the electrical resistance of unannealed piano-steel was tried, the stress used being eventually increased nearly to the breaking-point, with the view of ascertaining whether the increase of specific resistance which had been previously found to follow on moderate longitudinal traction might not be changed to decrease. This was not the case; but, on the contrary, whatever the permanent load might be, a temporary addition to this produced exactly the same temporary effect. The fresh experiments, however, confirmed the results obtained with the older ones, and agreed with these in making the alteration of resistance due to traction, both for unit stress per square unit of area and per unit increase of length, less for steel than for iron.

The electrical resistance of magnesium proved to be temporarily increased by temporary longitudinal traction of moderate amount, but the amount of increase was less than could be accounted for by mere change of dimensions, so that the *specific* resistance of magnesium, like that of aluminium, is *decreased* by the temporary stress. It was found also that *when the permanent load on the wire is very small*, both the temporary increase of length and the temporary increase of resist-

* See Sir William Thomson's paper on "The Electrodynamic Qualities of Metals," "Phil. Trans.," Part IV, 1856; also what follows.

† "Phil. Trans.," Part I, 1883, p. 152.

ance due to traction increase in greater proportion than the load. The increase of length, however, increases less rapidly than the increase of resistance, so that when the stress exceeds a certain limit, the above-mentioned decrease of specific resistance is changed to increase. Probably a similar state of things would be found to occur with aluminium.

The value of "Young's modulus" was determined for magnesium, both by the method of static extension and that of longitudinal vibrations, the results obtained by the two methods agreeing fairly with each other. The "simple rigidity" was determined by the method of torsional vibrations.

The value of the "bulk-modulus" was determined from the values of "Young's modulus," and the "simple rigidity" to be $287 \cdot 6 \times 10^6$ grams per square centimetre, and this value agreed with that deduced from a formula given in a former portion of this memoir* within 2 per cent. This formula is

$$e_v = 2071 \times 10^6 \times c_v^{\frac{7}{3}},$$

where e_v is the "bulk-modulus" in grams per square centimetre, and c_v the mean thermal capacity per unit volume between 0° C. and 100° C.

The results obtained from the experiments on the effects of stress on the electrical specific resistance of magnesium are in perfect accordance with the view that there is a relation between the "Hall effect" and the alteration of specific resistance.

The electrical resistance of the alloy platinum-iridium† is *much more increased* by temporary longitudinal traction than that of either of the components of the alloy. On the contrary the electrical resistances of platinum-silver, German-silver, and brass are all *considerably less increased* than is the case with their components. This circumstance would rather militate against the employment of platinum-iridium in making standard resistance coils. The increase of resistance alluded to above is much greater than can be accounted for by change of dimensions, so that the increase of *specific* resistance produced by longitudinal traction is considerably greater than is the case with any of those other metals examined whose resistance is increased by traction.

The alteration of resistance above mentioned at first increases in greater proportion than the load, but, when a certain limit of stress has been reached, the ratio of the temporary increase of resistance to the load producing it begins to diminish, and finally reaches the same

* Received by the Royal Society, May 28, 1884, and as yet only read in "Abstract."

† 90 parts by mass of platinum and 10 parts of iridium.

value as at first. A tendency to a similar state of things is seen with the other metals, but in none is the phenomenon so pronounced as in platinum-iridium.

The value of "Young's modulus" was determined for platinum-iridium, both by the method of static extension, and by that of longitudinal vibrations, and the results obtained were in fair agreement. "Young's modulus" is with the alloy considerably greater than with the two components, so much so as quite to equal if not to excel the value of the same modulus for piano-steel. The "simple rigidity" got by the method of torsional vibrations is also considerably greater with the alloy than with its components.

Unstretched platinum-silver was found to be thermo-electrically positive to the temporarily stretched metal, and forms a very striking exception to the rule which holds good for most metals, namely, that those metals which are *increased* to a comparatively large extent by traction in *specific* resistance are those which are rendered most thermo-electrically *negative* by traction.

A further examination was made of the "critical points" of metals, *i.e.*, points at which there is a sudden increase of the ratio of the permanent extension or permanent change of resistance produced by loading, and the load when the latter is increased by small and equal amounts at a time. This examination ended in the discovery of a *fourth* "critical point," occurring with a load exactly half of that at the previously so-called *first* "critical point;" further, it was ascertained that the four "critical points" are in the remarkably simple ratios 1 : 2 : 3 : 4. Table I gives the loads at the four "critical points," and their relation to "Young's modulus" for all the metals which have been as yet examined.

The above table shows not only that the four "critical points" are in the above-mentioned simple ratios, but also that with all metals the value of "Young's modulus" bears a constant ratio to the loads at each of the "critical points."

In Table II are given most of those results of the present inquiry which can be expressed by numbers:—

Table I.

Name of Metal.	"Young's modulus" in grams per square cm. $=e$.	Load at the first "critical point" in grams per square cm. $=c_1$.	Load at the second "critical point." $=c_2$.	Load at the third "critical point." $=c_3$.	Load at the fourth "critical point." $=c_4$.	$\frac{e}{c_1}$ $\times 10^2$.	$\frac{e}{c_2}$ $\times 10^2$.	$\frac{e}{c_3}$ $\times 10^2$.	$\frac{e}{c_4}$ $\times 10^2$.
Nickel..........	2175×10^6	$1\cdot158\times10^6$	$2\cdot200\times10^6$	$3\cdot100\times10^6$	..	18·79	9·89	7·02	
Iron............	1981	0·950	2·070	3·009	3825×10^6	20·86	9·57	6·38	5·17
Cobalt..........	1817	0·896	1·687	..	..	20·28	10·77		
Platinum........	1490	..	1·460	1·520	2021	..	10·64	9·80	7·20
German-silver ...	1291	0·637	1·274	2·197	..	20·20	10·14	5·88	
Copper..........	1174	0·607	1·165	1·886	2·625	19·35	10·08	6·23	4·35
Platinum-silver ..	1051	..	1·081	1·741	..	..	9·72	6·04	
Silver	742	..	0·819	1·250	2·185	..	9·06	5·94	3·44
Aluminium......	673	..	0·730	1·120	1·293	..	9·22	6·01	5·19
Mean.......	..	..	..	..	..	19·90	9·90	6·69	5·10

Table II.

Metal.	Condition.	Density.	Young's modulus in grams per square cm.	"Simple rigidity" in grams per square cm.	Specific resistance, *i.e.*, resistance in ohms of 1 c.c. between opposing faces for a temperature 16° C.	Alteration of resistance per unit for a longitudinal traction of 1 gram per square cm.*	Alteration of resistance per unit attending unit increase of length.	Alteration of specific resistance per unit attending unit increase of length.
Cobalt....................	Unannealed	8·231	2005×10^6	..	2450×10^{-8}	$-242·3 \times 10^{-2}$	−0·486	−1·986
Cobalt....................	Annealed	8·259	1817	..	2289	−386·8	−0·703	−2·203
Magnesium...............	Unannealed	1·743	430·8	$172·3 \times 10^6$	565	+1841	+0·781	−0·719
Platinum-iridium	Unannealed	21·523	2089	724·8	2830	+3049	+6·368	+4·486

* A + sign in this and the next two columns signifies *increase* of resistance on the *applicatio* of stress.

IV. "On the Variations of the Mean Diurnal Inequality of the Horizontal Component of the Earth's Magnetic Force at Bombay, and their Relations to the Sun-spot Period." By FREDERICK CHAMBERS, Acting Superintendent of the Colaba Observatory, Bombay. Communicated by C. CHAMBERS, F.R.S., Superintendent of the Colaba Observatory. Received October 9, 1884.

(Abstract.)

The investigation with which this paper deals was suggested by a remark made by Dr. Balfour Stewart, in "Nature," vol. xxiii, p. 238, to the effect that well selected magnetic observations might ultimately be found to indicate variations of solar heat more quickly and with greater certainty than any other kinds of indirect observations. The observations selected are those of the variations of the horizontal magnetic force, recorded at the Colaba Observatory, Bombay, between the years 1846 and 1880, comprising about 255,000 hourly observations. The mean diurnal variations for each month, calculated from *all* the observations, without any exclusion of disturbances, were extracted from the records of the Observatory; but instead of adopting, in the usual manner, the extreme range of these variations as the subject for further treatment, the mean of all the twenty-four hourly deviations, regardless of signs, was adopted; the objects aimed at in departing from the usual rule being to give due weight to all the observations, and to eliminate, as far as possible, the effects of rapidly fluctuating disturbances, without rejecting any of the observations. The monthly number obtained in the manner above described may be called the *mean diurnal inequality* for the month. The series of such monthly numbers should, after elimination of the annual variation, exhibit all those variations of the magnitude of the diurnal variation of the earth's magnetic force which may possibly depend on absolute variations of solar heat. Indications may therefore be thus obtained of all real variations of solar energy whose periods lie between one month and thirty-three years, if such there be. Several magnetic variations are shown to exist, and these are compared with the variations of the sun-spots.

The following is a brief summary of the principal results of the investigation:—

1. The mean diurnal inequality of horizontal force at Bombay is subject to a periodical variation, whose duration is almost exactly eleven years.

2. It is also subject to an annual variation, whose amplitude is

greater in the maximum years of the eleven-yearly period than in the minimum years of that period, the amplitude varying nearly in proportion to the mean diurnal inequality for the year.

3. It is also subject to a variation whose duration is more than thirty-three years.

4. If any other periodical variations than those above mentioned have any existence, their amplitudes must be small, or the duration of their periods must be less than one year, or more than thirty-three years.

5. The maximum and minimum values of the eleven-yearly period, and the range between them, have all increased in the successive eleven-yearly periods of the years 1846 to 1879.

6. The range of the eleven-yearly period is roughly proportional to the mean diurnal inequality for the interval from which the range is calculated.

7. The principal features of the eleven-yearly period coincide (except in point of time) with those of the well-known sun-spot period. The duration of the period is the same in both cases, and both variations exhibit a relatively rapid rise from minimum to maximum in about four and a half years, and a relatively slow fall from maximum to minimum in about six and a half years.

8. The magnetic curve *precedes* the sun-spot area curve by as much as six months.

9. The magnetic curve is more regular than the sun-spot area curve.

These or similar conclusions, with the possible exceptions of the last two, have either been enunciated by previous investigators, or are directly deducible from their labours; but the evidence now advanced is probably more decisive than any that has hitherto been published.

On the whole, the conclusions arrived at seem to support Dr. Stewart's views in a very unexpected manner, for not only does the eleven-yearly magnetic variation precede the eleven-yearly variations of certain meteorological elements, but it actually precedes, by several months, the eleven-yearly variation of the sun-spots themselves; and the probable error of a single monthly determination of a point on the magnetic curve is little more than 5 per cent. of the greatest range of the eleven-yearly oscillation, a degree of accuracy which is probably unapproached by any other kind of observations.

November 27, 1884.

Mr. J. EVANS, Vice-President and Treasurer, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election, was read as follows:—

President.—Professor Thomas Henry Huxley, LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Michael Foster, M.A., M.D.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.—Captain W. de Wiveleslie Abney, R.E.; William Henry M. Christie, Astron. Royal; Professor George H. Darwin, M.A., F.R.A.S.; Warren De La Rue, M.A., D.C.L.; Robert Etheridge, F.R.S.E., F.G.S.; Sir Frederick J. O. Evans, K.C.B.; Professor William Henry Flower, LL.D.; Professor George Carey Foster, B.A.; Sir Joseph D. Hooker, K.C.S.I.; Professor Henry N. Moseley, M.A., F.L.S.; Hugo Müller, Ph.D.; Captain Andrew Noble, R.A., C.B.; the Lord Rayleigh, D.C.L.; Professor J. S. Burdon Sanderson, LL.D.; Lieutenant-General R. Strachey, R.E., C.S.I.; Professor J. J. Sylvester, M.A., D.C.L., LL.D.

Professor Alexander Stewart Herschel and Professor Charles Stuart Roy were admitted into the Society.

The following Papers were read:—

I. "Notes on the Microscopic Structure of some Rocks from the Andes of Ecuador, collected by E. Whymper. No. V (Conclusion). Altar, Illiniza, Sincholagua, Cotocachi, Sara-urcu, &c." By Professor T. G. Bonney, D.Sc., F.R.S. Received October 29, 1884.

The number of specimens described in this concluding paper of the series is not great, although more mountains are represented than on any other occasion. Four out of the five mountains enumerated above are volcanic, and, as will be seen, their rocks all belong to the andesite group. Sara-urcu, however, consists wholly of metamorphic

rocks, and thus lies outside the volcanic zone. I am again indebted to Mr. Whymper for the interesting general notes which are prefaced to the lithological descriptions.

Altar.

"This mountain is probably the fifth in rank of the Ecuadorian Andes, and according to Messrs. Reiss and Stübel is 17,730 feet high. Owing to long continued bad weather I could neither ascend nor measure it. It is about 40 miles E.S.E. of Chimborazo, and lies S. of Cotopaxi, having nearly the same longitude.

"In the older maps of Ecuador this mountain is sometimes called Los Collanes, and there is a valley leading from it in a westerly direction still bearing that name. I encamped at the head of this valley close under the main mass of the mountain (12,540 feet) and in the neighbouring valley of Naranjal (13,050 feet) for five days, and experienced during the whole time a succession of storms of wind, rain, hail, and snow, and such observations as could be made were obtained with much difficulty.

"The peaks of Altar have been considered by previous travellers to fringe the edge of a *broken-down* crater. Appearances are in favour of this supposition, though there does not seem to be any distinct evidence in support of it. The valley of Collanes leads away from the open, or broken-down side, and if it were followed up to its furthest extremity the traveller would find himself in a vast *cirque* filled with glacier, and bordered by peaks rising to heights 4,000 feet above his level. The valley of Collanes is not at present occupied by such masses of *débris* as we should expect to find if there had been any great convulsion which wrecked the western side of the crater.

"The highest peak of Altar is situated at the head of the valley of Collanes, on its left bank or southern side. On the opposite side of the crater (*i.e.*, on the right bank or northern side of the valley) there are several points only slightly inferior in elevation to the highest peak; but at the extreme head of the valley (*i.e.*, the eastern side of the crater) the edge of the rim of the crater is considerably lower than upon its north and south sides. The summit of this depressed part might, in my opinion, be reached from the interior of the crater, but the highest point could not. Its face towards the interior of the crater is as steep as the most abrupt part of the Eigher in the Bernese Oberland, and it is covered with small hanging glaciers. From the exterior of the crater the summit of this peak may perhaps be reached. My assistant, J. A. Carrel, reported favourably upon it, stating that there was a large glacier upon its southern side (which was unseen from the valley of Collanes), and he brought back from the highest point he reached (which was probably about 3,000 feet above our camp) the specimens A and B, taken *in situ*, and C, a loose

fragment. My second assistant, Louis Carrel, who was sent simultaneously to report upon the northern side of the second summit, brought back from the highest point he reached (about 14,500 feet) the specimen D." (E. W.)

A. This is a very dark compact rock, with fairly numerous specks of a greyish felspar, and with occasional minute vesicles. Under the microscope the ground-mass is found to be a glass, in itself almost colourless, but so crowded with opacite as to appear almost opaque with low powers; in fact its true structure can only be seen in very thin sections and with high powers. In this also minute crystallites probably of a plagioclastic felspar abound. Crystals of the same mineral of larger size are also very frequent; the larger are often about ·02 inch diameter, or even more, but the smaller and more frequent are not much more than one-tenth of that size. The larger crystals resemble in all respects those so often described, and are probably labradorite; the smaller are lath-shaped, and appear to have smaller extinction angles, and so may belong to one of the species containing more silica. Besides these there are a fair number of crystals of augite, some very well characterised, some containing enclosures of brownish glass. There are also one or two crystalline grains, partly replaced by a secondary dark-brown mineral, which appear to me to be not improbably olivine. There are also some small round specks of a gummy-brown isotropic mineral. It is possible that these are amygdaloidal and a variety of palagonite. From the above description it is evidently a little difficult to decide whether to retain this rock in the augite-andesites or to term it a basalt, and a chemical analysis would be necessary to settle this point. It is evidently near the border line, but I prefer on the whole to class it with the augite-andesites.

B. This specimen is a rough scoriaceous lava, weathering a dull reddish to brownish colour; it is full of small cavities, but in the more compact portions little crystals of light-coloured felspar are common. It is evidently so closely related to A that, as the specimen is in a less favourable condition for examination, I have not had it sliced. Of the two I should more unhesitatingly name this one an augite-andesite.

C. A very dark compact rock containing fairly numerous specks of a light-coloured felspar. The specimen is a fragment of prismatic form, having six sides pretty clearly, though unequally, defined. Examined microscopically the ground-mass is seen to be composed of a mass of felted microliths of plagioclastic felspar (probably to a large extent oligoclase), with granules of augite and of iron peroxide, most of which are in all probability magnetite. The structure in short is one common in andesites and allied varieties of "trachyte." Probably a little glass remains unindividualised, but I have not been able to satisfy myself on this point.

Scattered in this, and belonging probably to an earlier stage of consolidation, are the following minerals: (*a*) plagioclastic felspar, often about ·02 to ·04 inch diameter, probably, as usual, labradorite. Many of these have a broken and corroded aspect, this being more than usually conspicuous; (*b*) magnetite; (*c*) grains of a pyroxenic mineral. It is rather difficult to decide upon the species of the last; some of the grains are prismatic in form, but others have a more or less rounded outline. The cleavage seems imperfect. The mineral is clear, colourless, and of a rather frosted texture. The imperfection of the external form and of the cleavage makes it difficult to obtain clear indications of the crystalline system. Brilliant colours are given with crossing Nicols. It is not impossible the mineral may be monoclinic, still some results suggest the probability that it is orthorhombic. If an augite, it is rather abnormal, but we must be prepared for the presence of some other magnesian silicate, such as olivine or (more probably) enstatite. Be this as it may, the rock itself is a member of the augite-andesite group.

The rock D, from the north side of Altar, has a different aspect, being (like some already described) a reddish-grey trachyte, studded with crystals of rather glassy white felspar, roughly about ·1 inch diameter, and containing some minute vesicles. It has not yielded a very good slide. The ground-mass appears to be a clear glass, with numerous lath-like crystallites of felspar, but is so thickly crowded with ferrite and opacite, especially the former, as to be all but opaque except in the thinnest part of the section. In this ground-mass are scattered crystals of the usual plagioclastic felspar, and a pyroxenic mineral; some of this is monoclinic augite, but I think a few grains are certainly orthorhombic. Thus the rock is an augite-andesite, probably containing some hypersthene.

Illiniza.

"This mountain is probably the seventh in rank of the Ecuadorian Andes, and is the most southern of the four which lie (approximately) in a line.* It bears exactly due west of Cotopaxi, at a distance of about 20 miles. The road to Quito passes over the ridge which in a manner connects the two mountains. According to Messrs. Reiss and Stübel its height is 17,405 feet. On February 9, 1880, I reached the height of 17,023 feet on the south side, upon June 9, 1880, the height of 16,922 feet on its north side, and in the middle of May, my two assistants, Louis and J. A. Carrel, gained the summit from the north.

"The summit of Illiniza proper (there is a secondary peak on the northern side which is termed Little Illiniza) is exceedingly sharp,

* The four (commencing from the north) are Pichincha, Atacatzo, Corazon, Illiniza.

and at the time of my partial ascents the crests of the ridges leading to the highest point were coated fantastically with overhanging cornices of snow and small glaciers. On the south side, such rocks as could be seen *in situ* were a highly decomposed trachyte, which externally bore an almost chalky-white appearance, with veins and patches of lilacs and purples pervading it. On June 9, 1880, previous to attempting to ascend the mountains from the north, I encamped against a huge block of trachyte (15,446 feet), from which I took the specimen A. The Carrels, on their ascent, brought from the highest rock visible, *in situ* (close to the summit), the specimen B, and also four specimens of *débris* from the same spot." (E. W.)

From Illiniza five specimens have been examined; one from the site of the camp on the north side, and four from the summit, whereof one was from rock *in situ*. The first, A, is a moderately dark-grey, slightly vesicular "trachyte," containing crystals of glassy felspar sometimes (though rarely) nearly $\frac{1}{4}$ inch diameter, and some small flakes of black mica. A slice shows a base of clear glass thickly studded with acicular microliths (probably in part at least felspar) and granules of ferrite and opacite, so that when viewed with low powers it has a brownish-grey, finely granular aspect. In this ground-mass are scattered abundantly crystals of felspar varying from about ·1 inch downwards, together with very numerous lath-shaped crystallites often about ·004 long. The larger, as usual, appear to be closely allied to labradorite. They have often externally a worn corroded or broken look; internally they vary much; some are nearly clear, many have a few microlithic enclosures or spots of brown glass, a few are very full of irregular patches of the latter; zonal banding is often well exhibited. There is a little brown mica, some very characteristic hornblende, also brown-coloured, and a considerable quantity of granules and small crystals of a pyroxenic mineral, some of which, I think, are monoclinic augite, but others appear to be orthorhombic, and are probably hypersthene. There are the usual granules of magnetite, and some minute colourless crystallites which may be apatite. The rock accordingly is a hornblendic augite-andesite, containing also some mica and hypersthene.

The specimens from the summit are more scoriaceous and of a redder colour. One (*débris*) is more compact than the others, and of a dull brick-red colour, spotted with small crystals of whitish felspar. The others are very similar, being a rough scoriaceous rock, containing felspar crystals similar to but not quite so large as those of the rock described above. In one is an irregular branching tube or vein, coated with a dull greenish or brownish glass, which I suspect to be a fulgurite. I have only had a slice cut from the specimen broken from the rock *in situ* (B). It is not in a good condition for examination, but consists, so far as I can make out, of a ferrite-stained glass, con-

taining crystals of the usual plagioclastic felspar, with a ferruginous mica, grains of hematite or magnetite, and one or more pyroxenic minerals, of which it is difficult to decide the species. The rock is an andesite, but perhaps it is safest only to prefix the epithet micaceous.

Sincholagua.

"This is probably the tenth in altitude of the Ecuadorian Andes,* and is situated about north-north-east of the cone of Cotopaxi at a distance of 11 miles, and lies nearly east of the town of Machachi (whence it is visible) at a distance of 13 miles. The culmination of this mountain is a long ridge, rising at its highest point into a very sharp tooth, to the summit of which we ascended, but were prevented whilst there from taking any observations by the occurrence of a severe thunderstorm. We made a precipitate retreat, remaining only long enough to secure a couple of specimens." (E. W.)

One of these, a portion of the actual summit, is a compact dark-coloured rock, with a slightly rough fracture, containing numerous small crystals of whitish felspar, generally not exceeding ·1 inch in the longer diameter. Under the microscope, the ground-mass is seen to be a felted mass of minute elongated crystallites, probably felspar, and of specks of opacite; there is probably a residual glassy base, but so numerous are the crystallites that it is by no means easy to be sure. In this ground-mass are scattered larger crystals of plagioclastic felspar similar to those already described, augite, with probably some hypersthene (but it is not abundant), and magnetite. The rock is thus an augite-andesite, probably hypersthenIferous. The other specimen taken *in situ* from the summit peak, a little below the highest point, is so evidently a rock of similar character, that I have thought it needless to have a specimen prepared for microscopic examination.

Cotocachi.

"This mountain, though probably slightly inferior in elevation to Sincholagua (and, consequently, only eleventh in order of the Ecuadorian Andes), presents a much more imposing appearance than its rank would lead one to expect. It is distant 45 miles in a direct line from Quito, being a little east of north, and from that city forms a prominent object. From the closer towns of Cotocachi, Otovalo, and Ibarra, above whose level it rises 8000 to 9000 feet, at distances of $8\frac{1}{2}$ to 15 miles, it wears a noble aspect. Reiss and Stübel by the mixed merc. bar. Δ method made its height 16,293 feet, and by merc. bar. observations on its summit I found its elevation to be 16,301 feet. Its summit ridge has two principal points, the lower being about 180 feet below the higher one.

* Height, according to Reiss and Stübel, 16,365 feet.

"There is no regularly formed crater upon the south, west, or eastern sides of Cotocachi. I cannot speak of the northern and north-western sides, which I have only seen from comparatively low ground at a considerable distance.

"In the neighbourhood of the mountain Cotocachi the surface of the earth is more fissured than any other part of Ecuador. The district about Cotopaxi, to which I have adverted when speaking of that mountain, alone presents anything comparable to the extraordinary series of chasms which seam or star the soil in the central portion of the flat land of the Province of Imbabura. From below, the traveller is made aware of their existence by occasionally having to skirt their sides for several miles, in a direction, perhaps, the very contrary of that which he desires to take; but their full extent is only perceived when one looks down upon the flat land from a considerable elevation, and then it is seen that the whole country in this vicinity is fissured by earthquake cracks, often several miles long, and sometimes hundreds of feet across.

"This neighbourhood has experienced very severe earthquakes during the last few centuries, the most recent happening shortly after midnight on August 16, 1868. Several persons independently pointed out to me a fissure, which had opened on this occasion, not less than 5 miles long, and varying in width from a few yards to several hundreds of feet. This earthquake was an affair of a few seconds, and in that brief interval almost the whole buildings within 10 miles were ruined, and tens of thousands perished. The priest of Cotocachi town informed me that that place was totally destroyed—not a single house being left erect. At Ibarra (about 13 miles distant from the mountain), Señor T. Gomez de la Torre, the greatest landed proprietor of this region, informed me that there were lists extant showing that 20,000 persons perished in that town alone—a statement that is scarcely credible, as the town of Ibarra has not the appearance of a place with so considerable a number of inhabitants. Respecting the widespread havoc which was caused on this single occasion, there is not, however, any question. The towns of Otovalo and Cotocachi had well nigh been rebuilt; but, at the time of my visit, most of the public edifices of Ibarra still remained in ruins, including schools, the hospital, a convent, and six churches, besides the church of the Jesuits, the finest building in the town, and the most imposing edifice I saw in Ecuador." (E. W.)

From this peak two specimens have been brought by Mr. Whymper—one from the summit rock, another from about 200 feet below it. The former is a purplish-grey rock, containing small whitish felspar crystals, with a good many minute vesicles. These have made the preparation of a slice a rather difficult task, and it is not so thin as I could desire. The ground-mass, however, appears to consist of a glassy

base, containing minute crystallites, probably for the most part felspar, but perhaps also a pyroxenic mineral, with rods of opacite and with ferrite staining. In this occur crystals of plagioclastic felspar, not generally exceeding ·03 inch, agreeing in general character with those already described, but perhaps more frequently containing enclosures, and "dirty looking," together with a pyroxenic mineral. The crystals of this are not very characteristic, but I think both augite and hypersthene can be identified. The other specimen is a small, rather rounded fragment, of a darker and more vesicular lava than the last, but with the usual whitish felspar crystals. The ground-mass, except in the thinnest sections, is practically opaque, but in those it is seen to be a light brown glass studded with crystallites, as already described, and containing specks of opacite. There are the usual larger crystals of plagioclastic felspar, perhaps rather cleaner than in the case of the other rock, with crystals of a pyroxenic mineral, among which, I think, both the monoclinic and the orthorhombic species may be recognised. Thus both these rocks from Cotocachi appear to be hypersthenferous augite-andesite.

Sara-urcu.

" Few references to this mountain will be found in the works of previous travellers in Ecuador. Reiss and Stübel appear to have *seen* it, for in their list of altitudes they mention that they made its height 6,800 metres (15,748 feet) by Δ in the year 1871, but they seem to have been doubtful about their determination, and do not enter that or any altitude in their "*Tabla comparativa.*" They quote from Villavicencio, who in his *Geografia de la Republica del Ecuador* (p. 52) gives its height as " 6,210 varas sobre el mar," upon which Messrs. Reiss and Stübel remark " es probable que estas alturas no son sino avaluaciones aproximativas ó reducciones inexactas; pero no resultados de trabajos originales " (reckoning the vara at 2·782 English feet, 6,210 varas are equal to 17,276 English feet).

" Villavicencio quotes from Velasco passages to the effect that this mountain is a volcano that formerly emitted fire and has latterly ejected ashes, which have produced consternation in Quito, from which city the mountain is distant 35 miles to the east. I give the original below.* The results of my observations show that the mountain is not a volcano; that it is only 15,502 feet high; that it cannot

* " Segun refiere el P. Velasco en su historia de Quito, este volcan ha arrojado llamas por dos veces; mas, en estos últimos años, ha arrojado gran cantidad de cenizas volcánicas, por Diciembre de 1843, i por el mismo mes, en 1856. La primera de estas erupciones duró dos dias, i puso en mucho consternacion á los habitantes de Quito, i á sus pueblos circumvecinos. La altura de esta montaña es de 6210 varas sobre el mar. . . . Está situado á 35 millas E. de Quito."—Villavicencis, pp. 52–53.

have emitted fire and ejected ashes; and that it lies to the east-north-east of Quito, at the distance of more than 40 miles.

"The examination and ascent of this mountain (which is the least in elevation of those which I explored) occupied a greater length of time and gave more labour than any other, excepting Chimborazo. It will be enough to state in these notes that as we proceeded to the east we travelled out of the volcanic region of the Ecuadorian Andes and entered the older formations, passing through dense vegetation, which for a time entirely obscured the rocks. We encamped several days under the first rock that we came across—a large projecting cliff of mica-schist, situated almost exactly upon the Equator, at the height of 12,000 feet above the sea, which bears the name *Corredor Machai:* and waiting until we could see Sara-urcu, which was almost perpetually enveloped in clouds, reached the summit at length on April 17, 1880. There was a rudely level ridge of some length at the summit, upon which a considerable amount of rock *in situ* was exposed, which left no doubt as to the nature of this mountain. The glaciers we traversed on the west of the peak possessed small but very distinct moraines, and from the moraine matter, which evidently had been borne from various rocks cropping up through the ice, I collected various other specimens of mica schists and quartz, but could not detect a single fragment of the characteristic rocks which you have described in connexion with the other mountains—the active and extinct volcanoes of the Equator." (E. W.)

The specimen from *Corredor Machai* is a rude flat slab of rock which, macroscopically, most resembles a rather fine-grained micaceous gneiss. The slaty formation is evidently due to a rough cleavage traversing the rock, on he planes of which a silvery mica has been rather largely developed. Under the microscope the specimen is seen to contain the following minerals:—A silvery mica in scales, up to ·03 inch long, probably a hydrous alkaline-mica; a dull green mica (or, perhaps, in some cases a chlorite), much less abundant; numerous granules of quartz, with rather even sides and rectilinear outline, a colourless mineral, sometimes rather dusty-looking from included granules of opacite. This appears to have two not very well marked cleavages (indicated often rather by a constancy in the direction of included mica-microliths), which gives with crossing Nicols rather "stronger" colours than quartz (to which it bears considerable resemblance). It is difficult to decide upon the true nature of this mineral—andalusite and kyanite are the two which suggest themselves—and as I can get no evidence of its being orthorhombic, I think it more probably a not very characteristic variety of the latter; there is apparently at one end of the slide a portion of a vein of a similar mineral in a less pure condition; the long-bladed crystals, so often characteristic of kyanite, do not occur. The structure also of the rock is perplexing;

it must undoubtedly be classed with the crystalline schists, but I suspect that the very marked schistosity is a secondary development due to crushing.

From the *débris* on the peak of *Sara-urcu* one specimen has been examined. It is a fairly fissile mica-schist consisting mainly of a silvery mica in rather small flakes, with apparently a quartzose or felspathic constituent. Under the microscope there appear to be two micas —one, far the most abundant, a rather light yellowish-green colour, the other colourless. There is a moderate amount of rather clear quartz, containing occasional small microliths, probably of the former mica, and very minute cavities. There is some calcite, which often includes a minutely granular earthy-looking material rather clustered together. The layers of mica exhibit a kind of foliation, but the actual flakes are often (though not always) athwart the layers; this, together with other indications in the slide, leads me to suspect that the rock subsequently to its foliation has been compressed in a direction making a considerable angle with the original foliation planes.

Another specimen is a rather friable schist, consisting chiefly of a dark green, or nearly black mica, with a finely granular green mineral, which I have little doubt is epidote; there is also a little of a silvery mica. A third specimen is a piece of a quartz vein, from a rock identical with the last mentioned. Three other specimens from Sara-urcu, one broken from rock *in situ* on the summit, were examined macroscopically by myself soon after Mr. Whymper's return, but have since been unfortunately mislaid. The summit rock appeared to me a rather fine-grained gneiss, containing quartz, felspar, dark mica, with probably a little chlorite and epidote. Another was a schist, in which I distinguished quartz, dark mica, and a silvery micaceous mineral resembling sericite. The third was a vein specimen from a rock corresponding with the last named. I remarked in a letter to Mr. Whymper at that time:—"These specimens prove the existence of true metamorphic rocks, which are in about the same condition as many of those in what is called the 'newer gneiss' series of the Scotch Highlands."

A specimen from the moraine on the west side of Sara-urcu has been examined microscopically. It is a dull-coloured rock, which consists of a micaceous mineral with silvery lustre, and a dark mineral in small grains, being evidently a rather minutely constituted member of the group of the spotted schists (fruchtschiefer). The lustrous aspect is visible on the faces of the rough divisional planes; on cross fractures there is little lustre, and the rock has a slightly friable aspect. Under the microscope, what we may call the ground-mass of the rock consists of films of a very pale brown, almost colourless micaceous mineral, felted together in wavy folia, and interspersed with numerous specks of ferrite and occasional small granules of quartz; in this are

little lenticular patches, some containing more or less of quartz and of a white mica, but in most the space is mainly occupied by two minerals —the one (probably the dark specks visible in the hand specimen) occurs in rather rounded grains, which are darkened by a dusty opacite and by minute specks of a yellowish mineral. It sometimes appears to exhibit a slightly "radial" structure, but often this is not distinguishable. It is not isotropic, though it only gives dull colours with the Nicols. Occasionally it appears to be bordered with small prisms of the mineral next to be described. This occurs in prisms of rather "bladed" aspect, commonly, as it seems, four-sided, often about ·02 or ·025 inch long, and about one-fourth to one-eighth the breadth. There is a clearly marked, though not frequent, basal cleavage, making a large angle with the sides. The mineral is a very pale greenish-yellow in colour, rather granular in aspect, contains occasionally enclosures of opacite, &c., and gives moderately bright colours with the Nicols. Though extinction takes place at a rather small angle with the sides of the prism, the mineral is clearly neither uniaxial nor orthorhombic. It is not unlike an epidote, but I do not think it belongs to this group; moreover, there is nothing in the macroscopic aspect of the rock to suggest the presence of epidote, the glimmering scaly mica and the rather lustrous spots of a black mineral being all that is visible with a strong lens. I suspect that these minerals are rather nearly related, and both belong to the group of anhydrous alumina silicates—varieties, perhaps, of fibrolite or kyanite. Unfortunately my collection is not well provided with specimens containing these non-alkaline alumina silicates, so that I am unable to give a more precise definition. The rock, however, evidently belongs to the "fruchtschiefer" group, and I should conjecture has come from rather high up in a metamorphic series, probably occupying a position above those already described.

It is then evident that, as Mr. Whymper asserts, Sara-urcu has no claim to be called a volcanic mountain. All the specimens which he has brought from it are metamorphic rocks. They do not, indeed, belong to the earliest types, such as the coarse gneisses of the Hebrides, but still they are greatly altered. Unless there were very clear evidence to the contrary, I should regard them as members of the Archæan series.* Humbolt ("Cosmos," vol. v, p. 44, Bohn's edit.) mentions the occurrence of "greenish-white mica-slate with garnets," at the foot of a "black trachyte," at Penipe, not far from the mouth of the Rio Blanco; further on, "at the hacienda of Guansce, near the shore of the Rio Puela," and probably below that schist, a granite of

* It will be remembered that in Bolivia and Peru the eastern (and here much the more elevated) chain of the Andes consists of sedimentary (silurian) rocks, the western (ill-marked) range being volcanic. See D. Forbes, "Quart. Jour. Geol. Soc.," vol. xvii, p. 7.

a middling grain, with light reddish felspar, a small quantity of blackish-green mica, and a great deal of reddish-grey quartz. Again, "further to the south and a little to the east of the road leading from Riobamba Nuevo (9483), . . . mica slates and gneiss everywhere make their appearance towards the foot of the colossal Altar de los Collanes, the Currillan, and the Parama del Antillo." From these and other instances, he concludes that the great volcanic mountains rest on a foundation of schists or gneisses.

Tonalite or Quartz-diorite.

This specimen was taken by Mr. Whymper from a boulder in the bed of the S. Jorge River, at the place where the ordinary track from Bodegas to Quito crosses the stream a little above the village of Muñapamba, about 1350 feet above the sea, and at the commencement of the ascent of the Pacific slopes of the outer range of the Andes. This was the only place where Mr. Whymper found any rock resembling a granite, and he nowhere saw it *in situ*. It is macroscopically a fairly coarse granite-like rock in which a whitish felspar and dark green hornblende are the more conspicuous minerals. Under the microscope it is seen to consist of the following minerals:—Felspar, which in the great majority of cases exhibits either zonal banding or polysynthetic twinning, and may, I think, be safely regarded as principally oligoclase; hornblende with well characterised cleavage and external angles; a brown mica, sometimes almost opaque, frequently in aggregated flakes; quartz in granules, as in a granite, containing many fluid-cavities from ·0001 to ·0003 inch, in which commonly are bubbles occupying about $\frac{1}{4}$ or $\frac{1}{5}$ of their volume, magnetite, or perhaps in some cases ilmenite, possibly a little apatite. The hornblende appears to have crystallised nearly simultaneously with the felspar, but perhaps a little earlier, the quartz last of all. The rock is in moderately good preservation. It has macroscopically and microscopically a considerable resemblance to the typical tonalite of the Adamello district.

Granite.

There is a small specimen of this rock of which Mr. Whymper writes:—

"At the town of Bodegas I found a specimen of granite in the possession of an old English resident in Ecuador, Mr. Wilson, who gave me a fragment from it. He told me that he had collected it *in situ* on the track leading from the town of Riobamba (9000 feet) to the village of Baños (5905 feet, R. & S.), but I could not learn at what altitude it was taken. The locality is in latitude a little north of the spot at which the last described specimen was collected, and is on the opposite side of the Andes, that is to say, on the Amazonian

slopes." The rock is macroscopically a rather coarse granite, not very well preserved. Microscopic examination shows that there is nothing exceptional in its composition. The felspar is a good deal decomposed, but orthoclase pretty certainly predominates, though a plagioclastic felspar is also present. There is a fair quantity of quartz (containing many cavities, decidedly smaller than those in the tonalite, which are usually empty) together with dark brown mica and green hornblende, and some magnetite.

Addenda and Conclusion.

The illustrious traveller Humboldt appears to have brought back specimens from some of the volcanoes visited by Mr. Whymper, and the results of an examination of these by Gustav Rose are given in "Cosmos" (vol. v, Bohn's edition). The latter refers the rocks of Rucu-Pichincha, Antisana, Cotopaxi, Chimborazo, Tunguragua, and those beneath the ruins of Old Riobamba to the fourth division of the trachytes, in which "the leading mass contains augite with oligoclase." Humboldt further states that he found hornblende in a separate or sporadic condition "in the rock of Pichincha," also, though not in large quantities, in the "trachytes" of Cotopaxi, Rucu-Pichincha, Antisana, and Tunguragua, along with "augite and oligoclase," but has only recognised it in two of the specimens from Chimborazo. The following table of silica percentages is also given (on the authority of St. Claire Deville) to which I have added those marked thus *.

Name of volcano.	Structure and colour of rock.	SiO_2.	
Chimborazo .	Semi-vitrified brownish-grey	65·09	Abich.
	Semi-vitreous and black	63·19	Deville.
	Crystalline, compact, grey	62·66	,,
Antisana	Grey-black	64·26	Abich.
	....	63·23	,,
	Vitreous grey*	77·76	Vom Rath.†
	Vitreous greenish-grey*	72·99	Teall.†
Cotopaxi	Vitreous and brownish	69·28	Abich.
	Granulated	63·98	,,
Pichincha ...	Vitreous black	67·07	,,
	Flesh-red*	62·99	Vom Rath.‡
	Dark*	64·55	,,
Puracé	Nearly bottle-green	68·80	Deville.

† Pitchstone west of Antisana.

‡ Of these two rocks one is "from the summit," the other from the crater; brief microscopical descriptions show that they are hornblende-andesites, with some augite; substantially identical with some I have described in Part I of these Notes.

The following analyses of the rocks from Chimborazo are also given by Humboldt, which I quote for comparison with the description given by myself in Part III of these Notes. The first of them very probably represents a rock nearly identical with that obtained by Mr. Whymper at 19,300 feet.

	I.		II.
SiO_2	59·12	....	65·09
Al_2O_3	13·48	....	15·58
Fe_2O_3	—	....	3·83
FeO	7·27	....	1·73
CaO	6·50	....	2·61
MgO	5·41	....	4·10
K_2O	2·64	....	1·99
Na_2O	3·46	....	4·46
Loss	—	....	0·41
	97·88		99·80

I. Broken from narrow rocky ridge at 19,194 feet; S.G. 2·806 (Rammelsberg).

II. Specimen collected at 16,179 feet; S.G. 2·685 (Abich).

Certain of these specimens appear to have been re-examined by Dr. Artope, and described in an inaugural dissertation published at Berlin in 1872; but of it I have only been able to see an abstract in Leonard's "Jahrbuch," 1874, p. 93. In this is given the analysis of a specimen from Pichincha, which, as it is described as having a greenish-black hard ground-mass, must be rather different from any in my collection. It was collected at a height of 15,539 feet; the specific gravity was = 2·624.

SiO_2	62·347
Al_2O_3	17·324
FeO	4·501
MnO	0·036
CaO	5·426
MgO	3·603
K_2O	3·126
Na_2O	4·286
H_2O	0·129
	100·778

It will be remembered that the species andesine has been formed for the reception of those plagioclastic felspars in the volcanic rocks of the Andes, which were termed oligoclase by the earlier observers. Some

of these have been analysed by Vom Rath in "Zeitsch. Deutsch. Geol. Gesell.," xxvii (1874), p. 296, and from this I quote two examples—

	I.	II.
SiO_2	58·15	59·10
Al_2O_3	26·10	26·10
CaO	9·05	8·35
K_2O	—	0·50
Na_2O	6·70	5·50
	100 00	100·55

I. S.G.=2·647. Loss on heating 0·27. From a spherulitic obsidian; Antisana.

II. S.G.=2·620. Loss on heating 0·01. From a dark andesite, containing among larger constituents hornblende, some augite, and plagioclase. From the crater of Guagua Pichincha.

It is evident that this felspar contains more silica and soda, and less lime than normal labradorite, of which in Dana's table of analyses (see "Mineralogy," s.v. Labradorite) the greatest amount of silica is given as 55·8, while the Na_2O more often lies below than above 5, and the CaO ranges from 8 to nearly 13, and rather usually exceeds 9 per cent. Still, as the name andesine is by no means universally accepted, and it is extremely difficult to say what constitutes a species in felspar, I have throughout designated these larger felspars in the Andes rocks as labradorite, with which in their optical characteristics they appear to correspond very nearly.

A comparison of this paper of Professor vom Rath's (which I had unfortunately overlooked when writing my notes on Antisana), with one by Professor Theod. Wolf in the "Neues Jahrbuch" (1874, p. 377) shows that the pitchstones examined by myself came from the same vicinity as that described by Professor vom Rath in the note to which I referred in Part II. Professor Wolf states that quartz-andesite (which it will be remembered does not occur in its normal condition in Mr. Whymper's collection) is found at Achupallas, on Antisana, and to the south of Riobamba, as well as at the volcano of Mojanda. It forms the whole hill of Achupallas (12,402 feet), and is largely developed on Antisana. He mentions among the localities Urcucuy and Tablarumi, and speaks of a wonderful perlite lava-stream as occurring on Urcucuy, a hill "im Paramo, nicht weit unter dem Antisana-kegel." It is, therefore, evident that the specimens of pitchstone brought by Mr. Whymper (obtained from a collector) came from this district, since they bear the label *Quebrada de Urcucuy*. Professor vom Rath gives an analysis of the glassy part of the rock (I), with a separate one of the spherulites (II).

	I. S.G. 2·32.		II. S.G. 2·386.
SiO_2	77·76	....	77·01
Al_2O_3	13·14	....	12·90
CaO	0·63	....	0·21
MgO	—	....	0·29
Fe_2O_3	1·47	....	1·88
Alkalies and loss ..	7·00	....	7·71
	100·00		100·00

The silica percentage in the specimen which was examined for me by Mr. J. J. H. Teall was not quite so high, being = 72·99; its specific gravity was 2·337.*

As the descriptions of these rocks from the Ecuadorian Andes have had to appear separately, it may be convenient to conclude this series of notes by a brief summary in a tabular form of the volcanic products of which I have described the microscopic structures.

It will also serve to impress upon us the general uniformity of the rocks occurring in the district. This was so obvious to Mr. Whymper, that he deemed it needless to bring many specimens from the mountains which he visited in the latter part of his journey. Yet the area which they occupy is a large one. From Altar and Chimborazo, the most southerly pair, which lie on an E.S.E.—W.N.W. line, about 40 miles apart (passing by Carihuairazo) to Cotopaxi and Illiniza (on nearly the same parallel of latitude) is rather more than 60 miles. Next come Corazon, Sincholagua, and Antisana (also roughly on an east and west line, the first being not much less than 40 miles from the third). Passing northward by Pichincha (N.W. of Quito), we leave on the east, Cayambe, which is a few miles east of the longitude of Antisana, and finally reach Cotocachi, the most northerly volcanic summit, having traversed a zone whose extreme length cannot be less than 130 miles from N. to S., and whose greatest breadth from east to west must be about 40 miles.† If, as some think, volcanic rocks are but sedimentary deposits melted down, this general uniformity is rather remarkable.

* Zirkel ("Microsc. Beschaff," p. 448) says that he has not found free quartz under the microscope in the so-called augite-andesites, but I do not understand whether he includes among them the rocks mentioned by Professor Wolf as occurring on the west side of Antisana. The composition and structure of the pitchstones described above make the occurrence of dacite in the highest degree probable, but I have not myself noticed free quartz in any of the ordinary Andes rocks. I regret that by an oversight "former" was written for "latter" at the end of the article on Antisana. I consider the pitchstone of Urcucuy, as stated in the body of the article, to be more probably allied to dacite than to rhyolite.

† The mountains are thus grouped by Mr. Whymper (reckoning from north to south)—*Western Andes:* Cotocachi, Pichincha, Corazon, Illiniza, Carihuairazo, Chimborazo. *Eastern Andes:* Cayambe, Antisana, Sincholagua, Cotopaxi, Altar.

Name of Volcano.	Pitchstone.	Hornblende-andesite.	Augite-andesite.			
			Ordinary.	Hyperstheniferous.	Hornblendic.	Micaceous.
Pichincha—						
Crater of Guagua-Pichincha, with actual summit	..	×				
Rucu-Pichincha, with actual summit	..	..	..	×		
Other parts of the mountain	..	..	..	×		
Antisana—						
Highest rocks (16,000 feet)	..	..	×	×		
Antisanilla	..	..	×			
Guagra-ialina	..	..	×			
Urcucuy	×					
Other localities, west side	..	× *	..	×		
Cotopaxi—						
Summit	..	..	..	×		
Camp, 15,100 feet	..	..	..	×		
Chimborazo—						
Highest rock (19,300 feet)	..	..	×			
At 18,400 feet	..	..	..	×	×	
17,300 „	..	..	..	× †		
16,000 „	..	..	..	×		
15,950 „	..	..	× ‡	× †		
Carihuairazo—						
Summit	..	..	× §			
Cayambe—						
Summit	..	..	..	..	× ‖	× ‖
Pointe Jarrin	..	..	..	..	×	
Corazon—						
Summit rocks	..	..	×	×		
Altar—						
Main peak at about 15,500 feet	..	..	×			
Second peak at about 14,500 feet	..	..	..	×		
Illiniza—						
Summit	..	..	..	..	..	×
Camp, north side, 15,446 feet	..	..	..	..	× ¶	
Sincholagua—						
Summit	..	..	×	× (?)		
Cotocachi—						
Summit	..	..	..	×		
About 200 feet below summit	..	..	..	×		

* Contains also mica.
† Hypersthene predominating.
‡ Rather abnormal.
§ Perhaps hyperstheniferous.
‖ Poor in augite.
¶ Contains also some mica and hypersthene

II. "Experiments to Determine the Origin of the Respiratory Sounds." By J. F. BULLAR, M.B. Cantab., F.R.C.S. Communicated by Dr. LAUDER BRUNTON, F.R.S. Received October 30, 1884.

A detailed account of the various existing theories of the production of the respiratory sounds may be found in Dr. Paul Niemeyer's "Handbuch der Percussion und Auscultation" (Erlangen, 1870).

The various theories, though differing in minor points, may be arranged under three principal heads.

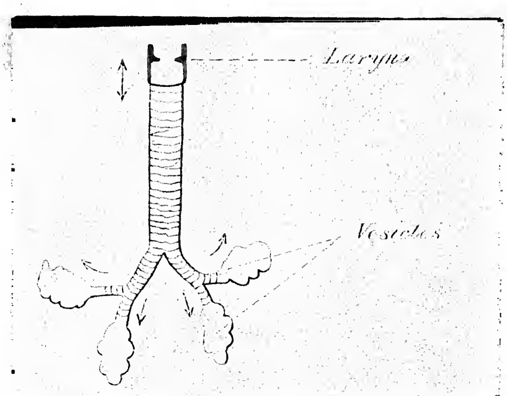
A. According to the first the sounds are produced all along the respiratory tract by the friction of the air against its walls.

B. According to the second the sounds are produced at the glottis alone, and the differences in the sounds heard over the trachea and lungs are attributed to the greater or less conducting power of the structures through which they are heard at each spot.

C. According to the third theory the sounds are produced at those parts of the respiratory tract where the air passes from a narrower to a wider space. Thus during inspiration one sound is produced at the glottis, and another at the points where the smallest bronchioles open into the vesicles. During expiration a sound is produced at the glottis only.

The sounds are represented diagrammatically in the figure.

FIG. 1.

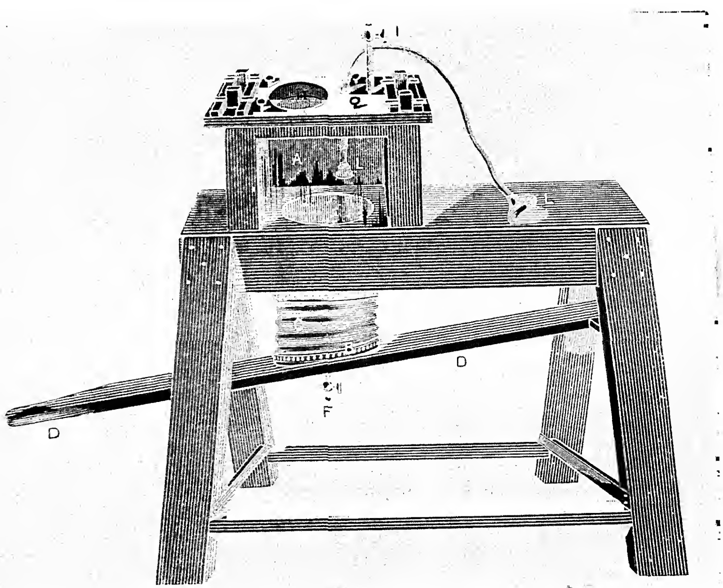


The arrows show the direction of the air-current by which a sound is produced at each spot.

As neither of the above theories is universally accepted, the experiments about to be described were undertaken in the hope of determining in what parts of the respiratory tract the breathing sounds are produced, and of deciding which, if any, of the above-mentioned theories is the true one.

The experiments were, for the most part, performed with the artificial thorax represented in fig. 2. This consisted of an air-tight

Fig. 2.



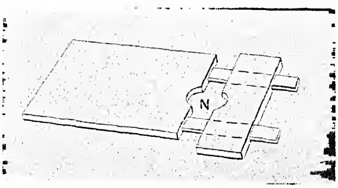
chamber A, with glass sides fixed upon a firm wooden bench. From the chamber A, a large smooth hole passed through the bench into a secondary chamber C, the sides of which were made of flexible india-rubber cloth, like a bellows. The upper edge of the india-rubber was fastened air-tight to the lower surface of the bench round the hole leading into the chamber A. The bottom of the bellows B was attached to the handle D, moving on a hinge at E. At the bottom of the bellows was the tap F.

Near the middle of the roof of the chamber was a hole H large enough to admit a sheep's or calf's lung when collapsed. A tap I and the tube of the flexible stethoscope L also passed through the roof, the inner end of the stethoscope being covered with bladder to prevent the escape of air or water through it.

The lower tap being shut, and water poured into the chamber till it was nearly full, the artificial chest was ready to receive the lungs.

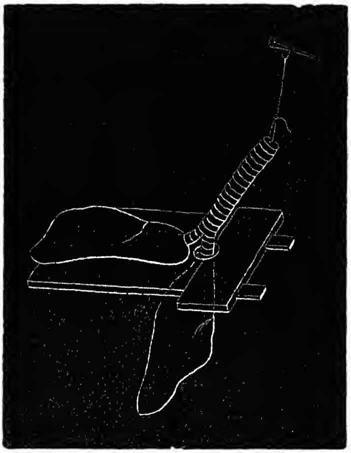
The right lungs of calves and sheep have a small supernumerary bronchus arising from the trachea about 2 inches above its bifurcation. The left lung was therefore always chosen to be placed in the chamber, and this was accomplished by passing the lung between the two parts of the sliding frame shown in fig. 3, and then bringing the parts

Fig. 3.



together, so that the left bronchus passed without pressure through the hole formed by the two notches in the sides of the frame at N. One lung was then above and the other below the frame, as shown in profile in fig. 4.

Fig. 4.



The inner end of the stethoscope L was attached to the lower lung by tying it to the fold of pleura running down the back of the lung, and the lung and stethoscope were passed into the chamber through the hole in its roof.

The frame (fig. 3) was screwed down air-tight upon a ring of india-rubber tube placed round the hole in the roof of the chamber A, and the interval between the left bronchus and the frame stopped up by wrapping some tow, dipped in a strong solution of gelatine, round the bronchus.

The whole chamber containing the lung was now air-tight, and by opening the upper tap I and raising the handle of the bellows, any air remaining above the water in the chamber or any superfluous water could be expelled.

When the tap I was shut, moving the handle up and down caused the lung within the chamber to breathe, and by fixing the handle in different positions the lung could be kept motionless at different degrees of distension.

The chamber thus represented a thorax in which there was a sufficient amount of fluid to separate the surfaces of the lung and chest wall, and in which the respiratory movements were entirely diaphragmatic.

The motion of the machine itself and of the water contained in it produced no sound.

The object and results of the experiments about to be described will be made clearer by introducing here a short account of the respiratory sounds, and as the changes in the sounds produced by alterations in the physical condition of the lungs throw light upon their origin, a short account of the more important of these changes is also necessary.

The sounds to be considered may be divided into those of speech and those of respiration.

A. The sounds of speech are produced in the mouth and larynx. They are heard loudly and distinctly by the stethoscope over the larynx and trachea; over the parts of the chest in contact with the vesicular structure of the lungs they are weaker and indistinct.

In the neighbourhood of the large air passages, *e.g.*, in the inter-scapular space, the sounds are less weak and indistinct than at other parts of the chest, and approach more nearly in character to the sounds heard over the trachea.

B. The origin of the respiratory sounds is a disputed point.

Over the trachea during both inspiration and expiration a rushing sound is heard.

Over the lungs a sound is heard during inspiration of a different character from the tracheal inspiratory sound, and during expiration the sound is either very faint or altogether absent.

As in the case of the voice there may be, in the neighbourhood of the large air passages, an approximation to the tracheal sounds.

The sounds heard over the lungs are known as the sounds of "vesicular breathing."

Changes in the physical condition of the lungs alter the sounds either in intensity or in character, or in both, or the sounds may be altogether absent from parts where they are normally heard. Over parts of the lung which have become consolidated, or destroyed and replaced by air, and in certain other conditions, the normal sound of vesicular breathing is replaced by an inspiratory and expiratory sound, having more the character of the sounds heard over the trachea. These altered sounds differ in character according to the physical changes in the lungs, but they have the common feature distinguishing them from the sounds of vesicular breathing, that the expiratory sound instead of being much weaker is equal to or more intense than the inspiratory sound.

Sounds having this peculiarity are classed together as "bronchial" sounds; they resemble more or less the sounds normally heard over the trachea.

Many different qualities of bronchial breathing may be heard and have received different names, but for the present purpose, it will be sufficient to remember their common feature, namely, that the expiratory sound is as loud or louder than the inspiratory.

When the lungs are partially consolidated or otherwise changed, an alteration of the vesicular sound is often noticed over the unaffected parts of the lungs. The alteration consists in an increase in the intensity of both the inspiratory and expiratory sounds.

When the vesicular sound is thus intensified it is said to be "puerile" in character, the vesicular breathing of a child being louder than that of an adult.

Besides the above alterations in the breathing sounds, certain accessory sounds, such as those due to fluid in the air passages, and to friction of the pleural surfaces, &c., may be present.

The sound of the voice heard over physically altered lung differs both in intensity and in character from the sound heard over unaltered lung, and so far as this change does not correspond to any alteration in the voice sounds produced in the larynx and mouth, it must be due to changes in the conducting power of the parts through which the sounds are heard.

With regard to the changes in the breathing sounds the case is not so simple, for, as we do not know where the normal sounds originate, it is impossible to say how much the changes due to altered conduction may be supplemented by changes in the sounds produced.

In the rest of this paper the terms vesicular, bronchial, and puerile breathing will be used, in the sense just indicated, in order to save the constant repetition of descriptions of the sounds.

When the left lung is placed, as described above, in the artificial thorax, the right lung lies upon the top of the chamber collapsed, and is unaffected by the respiration going on in the left.

The trachea must be kept straight, and placed so that the air can freely pass in and out of it.*

The sounds heard in the two lungs are quite different.

Listening to the inner lung with the flexible stethoscope the breathing sound is quite distinct, and resembles vesicular breathing, the expiratory being very faint compared with the inspiratory sound.

Altering the size of the opening of the trachea does not sensibly affect this sound unless the narrowing be carried so far as to interfere with the entrance of air, when the sound becomes more feeble.

In the outer lung, in which the air is motionless, both *inspiration* and *expiration* are loud, the breathing is bronchial.

The sound is loudest over the larger bronchial tubes, but it is quite distinct as far as the edges of the lung. On compressing the bronchus leading to the lung the sound ceases to be heard.

If the trachea be plugged while the inner lung is in a state of inspiration and the machine kept in motion, both lungs breathe, expiration and inspiration alternating in the two.

The breathing sound in the outer lung now becomes "vesicular," changing to bronchial whenever the trachea is opened, and it ceases to breathe. The same result follows whether the trachea be left long or cut off and plugged close to the bifurcation.

These experiments show that the vesicular murmur is not produced in the glottis or trachea alone, since it continues when the larynx and nearly the whole of the trachea are removed, and when the trachea being plugged and the lungs breathing from one to the other, there is no current of air through what is left of the trachea.

With regard to the bronchial sounds in the outer non-breathing lung, it is well known that a current of air passing over but not entering the mouth of a tube produces a sound, which, under certain circumstances, may become a musical note, and that in cases where the sound is a noise rather than a note, that by listening carefully a note may be detected through the rushing noise. This is exactly the character possessed by the sound of bronchial breathing; we easily recognise, in different cases, notes of different quality and pitch.

In the non-breathing lung of the experiment, or in a case of morbid consolidation, this condition is realised, the air-current passes down

* The butchers usually remove the larynx from the trachea before the lungs are sold, and in the following experiments the larynx was absent. There were three reasons for my not objecting to this:—

1st. A dead larynx is a very different thing from a living one.

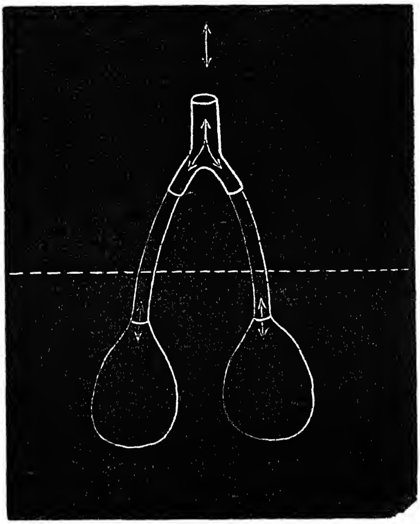
2nd. The vocal cords of calves and sheep are less developed than those of man.

3rd. A murmur is produced at the cut end of the trachea. It will be seen in the sequel that the reasoning of the present paper is unaffected by the absence of the larynx.

the trachea to the healthy parts of the lungs, and on its way blows across the mouths of the bronchial tubes leading to the consolidated parts without entering them.

There is, therefore, an *à priori* probability that this may be the origin of some part of the sound of bronchial breathing, and in order to determine whether it is so, the following experiment was tried:—

FIG. 5.



A Y-tube of gutta-percha, the stem about one inch and the arms about half-an-inch in internal diameter, was made to represent the lower part of the trachea, and its bifurcation into the two bronchi. The arms were each continued by an india-rubber tube about six inches long, and to the ends of the india-rubber tubes equal sized bags of gutta-percha tissue were attached as represented in the figure. The inside of the whole apparatus was smooth.

The artificial thorax being filled with air, the tubes, with the bags attached, were let through the roof of the chamber as far as the dotted line in the figure.

At each downward and upward stroke of the bellows air entered and escaped from the bags through the tubes, as shown by the direction of the arrows. The movement of the bellows was so regulated as to completely fill and empty the bags at each stroke.

During the inspiration and expiration thus caused, a feeble sound was heard, no doubt produced at the open ends of the tubes, the loudness of the sounds depended upon the force with which the

respirations were performed, and by working with a proper force the sound could be made very faint indeed. If, when this was the case, one of the smaller tubes was nipped, so as to prevent any air passing through it, while continuing to work the bellows with the same force, the sounds became greatly increased.

The expiratory sound was increased more than the inspiratory, and at the same time the character of the sound changed, a note could be detected in it whose pitch varied with the part of the tube pinched, the shorter the length of tube between the bifurcation and obstruction the higher being the pitch.

The sounds could be heard either at the mouth of the large tube (trachea) or with the stethoscope in the obstructed and unobstructed tubes (bronchi).

In this experiment if the bellows be worked at a uniform rate, the rate of the air-current in the large tube will not be appreciably affected by the stoppage of one of the smaller tubes. (If altered it will be diminished on account of the slight increase of friction.) The rate of the air-current through the trachea being unchanged by the stoppage of a bronchus, the increase of sound cannot be produced in the glottis. We must seek therefore in some part of the respiratory tract below the trachea for the origin of the sound caused by bronchial obstruction. The stoppage of one bronchus, although not affecting the rate of the air-current in the trachea, doubles its rate in the free bronchus, for when one bronchus is shut all the air entering the trachea passes through the free bronchus, and *fills* the bag (lung) belonging to it in the same time in which the two bags would have been *half-filled*, had both the bronchi been free. The stoppage of one bronchus not only increases the rate of the air-current in the other, but produces conditions for the development of a new sound at the bifurcation of the trachea, for as the air passes along the free bronchus it blows across the mouth of the obstructed tube, and a current of air crossing the mouth of a tube produces a sound. A sound produced in this way, like the sound in the experiment, varies according to the length of the tube blown across, a rushing noise is produced at the mouth of the tube, and the tube, according to its length, brings into prominence some particular note present in the noise, and so modifies its character.

By working the bellows so as to completely fill and empty the bags at each stroke, the same *amount* of air is made to pass through each small tube, whether the other be shut or open, but when one tube is shut the whole act of inspiration or expiration comes to an end in half the time occupied when both the tubes are open.

The important points to remember are—

First, that the obstruction of a bronchus upsets the ratio normally existing between the rates of the air-current in the different parts of

the respiratory tract. The cross section of the respiratory tract increases from the trachea to the vesicles, just as the cross section of the circulatory system increases from the aorta to the capillaries, and the rate of the air-current varies accordingly. When a larger or smaller bronchus is obstructed the cross sections of the bronchi and vesicles are more or less diminished, and the rate of the air-current in them proportionately increased, the rate in the trachea being unaffected by the obstruction as such.

And, secondly, that when a bronchus is obstructed a sound is produced by the air passing over its mouth.

The diminution in the time occupied in inspiration and expiration, brought about by the obstruction of a bronchus, is probably one cause of the increased frequency of respiration in cases where the lung is consolidated or compressed.

With regard to the theory of the laryngeal production of bronchial breathing, it is to be observed that in a case of complete consolidation of one lung each respiratory act must be performed *in less than half the normal time* before any increase in the rate of the air-current in the larynx and trachea, and therefore of the sound produced in them, can occur.

Repeating the above experiment with calves' lungs, the same result was obtained. Both lungs were placed in the artificial thorax with the trachea and main bronchi above the cover. On compressing either of the bronchi a marked increase of sound was heard over the trachea, the increase in the expiratory sound being as before the greatest. The shortness of the main bronchi made it impossible to elicit different notes by compressing them at different points; but it is probable that a partial consolidation by obstructing the smaller tubes would produce a different note from that obtained by stopping the air-current in the main bronchus.

The explanation of the fact that the expiratory is increased more than the inspiratory sound appears to be the following:—

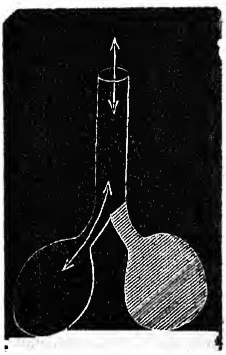
During both inspiration and expiration a sound is produced by the passage of the air over the mouth of the obstructed tube. During inspiration this is the only new sound produced, the air passing from the trachea to the bronchus, that is, from a larger to a smaller tube, as shown in fig. 6, but during expiration the air passes from the bronchus to the trachea—from a smaller to a larger tube—which, as is well known, is a sufficient cause to produce a sound. Therefore during inspiration a cause for the production of *one*, and during expiration causes for the production of *two* new sounds are present.

The passage of the air from the *two* smaller tubes into the larger ones does not produce any considerable sound, for when they are both open the expiratory and inspiratory sounds are much alike.

It appears to me that in these experiments the conditions present

in the chest in health and in consolidation of the lung are represented accurately enough for the purpose, and that they illustrate the way in which bronchial breathing may be produced in extensive consolidation, and in any other conditions which give rise to a stasis of air in the bronchial tubes.

Fig. 6.



In such cases a new sound is produced at the junction of the obstructed with the unobstructed tubes. This sound does not exist in health.

If the respiratory organs themselves and the structures covering them were good conductors of sound, the sounds produced at any part would be heard at all other parts, and wherever we listened on the surface of the chest or throat, we should hear the combination of all the sounds produced at the time in the different parts. For the distance between the most remote parts of the respiratory tract is so small that none but very feeble sounds would be entirely lost on that account, even if it were not the case that in a great part of this short distance the sound is conducted through tubes (trachea and larger bronchi), by which the effect of distance is diminished.

Of course each sound would bear a somewhat greater or less proportion to the whole combination, as the point ausculted was nearer to or farther from the seat of its production, and thus there would be a difference of character in the resultant or combination sound at different parts; it would, however, at all parts be a combination of the sounds produced in the different parts of the tract. The rate of the air-currents is so slow as to have no influence on the conduction of sound.

The bronchial breathing produced in the artificial system of tubes was heard equally well in all the tubes. In the body, however, this is not the case, the bronchial breathing heard over a consolidated lung is not recognised on the healthy side, owing to the feeble con-

ducting power of distended lung; for the same reason the voice is heard less plainly on the healthy side. In the living body we are listening to the sounds over *a distended lung in motion and permeated by air-currents*, and in which possibly sound is being produced.

In order to test the conducting power of *distended lung at rest*, the following experiment was performed.

A pair of lungs were arranged in the artificial thorax in the way described in the first experiment, and in addition a large india-rubber bag filled with air was attached to the trachea by means of a tube. By keeping the handle of the bellows fixed in any given position the lung within the chamber could be kept for a short time at any desired degree of distension, and by pressing at intervals upon the bag, air could be forced to and fro between the bag and the lung outside the chamber, without distending the lung within it. When this was done the sound in the outer lung was vesicular in character, that in the inner non-breathing lung bronchial; but the intensity of the sound in the inner lung varied with its amount of distension, being loudest when it was collapsed, and much fainter but still bronchial when it was nearly fully distended.

If, while the outer lung was being made to breathe from the bag, the handle of the bellows was allowed to move, the two lungs breathed together, and the sound in the inner lung at once increased, inspiration becoming much louder than expiration, and the sound acquired the vesicular character.

This experiment proves that the glottic and bronchial murmurs, and any other sounds elsewhere produced, are heard but faintly through a distended and motionless lung, and that their resultant has the bronchial character, expiration being heard as plainly as inspiration; and further, that the vesicular murmur is developed during the distension of this lung still further by means of a current of air. The only tenable explanation of these facts is that the vesicular murmur is produced in the lung itself; for if it be not produced in the lung it must be a conducted sound, and we have just seen that expansion of the lung diminishes its conducting power; but the peculiarity of the vesicular sound is that it is heard loudest during inspiration, that is to say while the lung is expanding, and by so doing becoming a worse conductor of sound. The sound is loudest when the conducting power of the lung is least; it cannot, therefore, be a conducted sound.

The sound heard over the distended but motionless lung was the combination of all the sounds produced in the respiratory tract greatly reduced by bad conduction, and consisted of sounds produced in the glottis, at the bifurcation of the trachea, and in the substance of the breathing lung.

In the present experiment the glottic sound was represented by the

sound produced at the junction of the trachea with the air bag. The origin of the sound at the bifurcation has been discussed above. That a sound is produced in the breathing lung is proved by the experiment; it seems probable that its cause is the passage of the air from the small bronchi into the vesicles.

In the living body, when there is stasis of air in a bronchus, the new sound formed at the mouth of the bronchus must be added to the pre-existing glottic and pulmonary sounds, and is no doubt one factor in the production of puerile or exaggerated breathing; the increased rapidity of motion of the air in the healthy lung being another.

With regard to the production of the vesicular or as it may now be called the pulmonary sound; if a lung be partially inflated and the bronchus tied it may be made to expand and contract freely by placing it in an air-tight vessel completely filled with water, and communicating by means of a flexible tube with a vessel containing mercury. Raising or lowering the mercury causes the lung to contract and expand. Under these circumstances there is no current of air through the bronchi; as the vesicles are more extensible than the bronchial tubes, a slight current must enter them during expansion, but this appears to be insufficient to produce a sound, as none could be heard with certainty through a stethoscope applied directly to the lung. The experiment shows that the pulmonary sound is not due to the movements of the tissues of the lung.

The results of the experiments now described are in accordance with the third theory quoted at the beginning of this paper, and the results of the vivisections performed on horses by Chauveau, Bondet, and Bergeon are confirmed by my experiments, which show that the vesicular sound is produced in the lungs, and not in the glottis or trachea.

The demonstration of the production of a new sound in bronchial obstruction I have not seen elsewhere.

III. "Note on the Origin of the Suprarenal Bodies of Vertebrates." By W. F. R. Weldon, B.A. Communicated by Professor M. Foster, Sec. R.S. Received October 30, 1884.

In the early part of this year I was enabled, by the courtesy of the Royal Society, to examine some specimens of *Bdellostoma Forsteri*, collected by Mr. Sedgwick by means of a grant from the Society. As a result of this examination, it appeared that the head kidney, described by Johannes Müller as connected, on the one hand with the segmental duct, and on the other by means of a branch of segmental tubules with the pericardium, had become modified in a very peculiar manner. The

connexion with the segmental duct, and the one or two glomeruli (both shown by Wilhelm Müller to exist in the embryo of Myxine) had disappeared, all that remained being a plexus of tubules, opening into the pericardium, but without any other opening whatever, and entirely unconnected with the rest of the kidney. This plexus was closely surrounded by a network of blood-vessels, and the larger tubules contained altered blood-corpuscles.

In seeking for a parallel, among other Vertebrates, to this peculiar modification of a portion of the kidney, I was struck by the following facts:—

In Teleostei and Ganoids, Balfour has shown* that the head kidney is often replaced, in the adult, by a mass of lymphatic tissue, richly supplied with vessels. Emery has studied the development of this tissue; he finds in the embryo of many Teleostei, at an early stage, a single pronephric funnel, communicating with the segmental duct; the rest of the "intermediate cell-mass," being an undifferentiated blastema surrounding the duct. In later development, only a portion of this tissue becomes converted into renal tubules; the rest remaining through life as "lymphatic" tissue, richly supplied with vessels.†

Now the Teleostei and the Cyclostomes are the only Vertebrates devoid of suprarenals; and where these latter organs exist, they are always closely connected with the kidney, having, in reptiles and amphibians, a "vena portæ," corresponding to the "renal portal system" of the kidney, and being in male reptiles situated in the mesorchium, side by side with the longitudinal network of the testis.

It therefore seemed worth while to re-examine the development of the "cortical" part of the suprarenal bodies. This I have done in *Pristiurus*, in *Lacerta*, and in the chick, finding it to be derived in each case from a portion of the mesonephric tubules.

In *Lacerta muralis*, the segmental vesicles, when they have become connected by the usual ∾-shaped cords with the Wolffian duct, become invaginated, and form glomeruli. In the region of the future generative ridge, as each glomerulus begins to invaginate, before a blood-vessel enters it, its mesenteric border (that opposite to the connexion with the ∾-shaped segmental tube) thickens, and becomes more than one cell thick,—the rest of its wall being never, at any period of its existence, formed of more than a single layer of cells. As the generative ridge develops, the thickened portion of each glomerulus increases, and gives rise to a process divided into two branches, one dipping ventralwards into the generative ridge, the other going dorsally between the kidney and the cardinal vein. Both branches are at first histologically indistinguishable, being alike composed of rounded cells, pressed closely one against another.

* "Quart. Journ. Mic. Sci.," 1882, and "Phil. Trans.," 1882.

† "Atti dell' Academia dei Lincei," 1882.

After a time, the dorsal branch becomes separated from the rest, being cut off by numerous small veins, which divide its substance into irregular trabeculæ. Its component cells also stain less easily than those of the ventral branch of the glomerular process. These dorsal branches are, as shown by their position, and by their subsequent union with processes from the neighbouring sympathetic ganglia, the rudiment of the cortical portion of the suprarenal body, the ventral processes forming the longitudinal network of the testis, already described by Braun.

The close contact which exists, in the later stages of development, between the suprarenal rudiment and the various blood-vessels which run through it, has led previous observers to assume that the blastema itself originates by proliferation of the walls of those vessels. The fact that the glomerular processes already mentioned may be easily recognised before the existence of these vascular branches, together with the great clearness and distinctness of the endothelium of the latter, which may always be seen as a sharply defined layer lying on the suprarenal substance, completely negative this view.

The later behaviour of the cortical blastema, and its union with the sympathetic, has been correctly described by Braun.

In the *chick* the development is, as might be expected, much modified. About the end of the fifth day numbers of large, rounded, deeply staining cells appear in the tissue between the vena cava and the mesonephros. These cells unite, during the sixth and seventh days, into clusters and finally into chains which acquire, on the eighth day, a connexion with the epithelium of adjacent glomeruli. After this they behave in a manner practically identical with that of the corresponding cords in the lizard.

As in the lizard, so in the chick, the cells in question are from the first so perfectly distinct from the walls of the adjacent vessels that they can by no possibility be supposed to have been budded off from them.

In *Pristiurus*, the suprarenals are, as is well known, segmentally arranged along the dorsal wall of the cardinal veins. Balfour describes their cortical tissue as arising from an impaired rod of mesoblast, lying at the root of the mesentery. As to the origin of the rod itself, he says nothing. Its actual origin I believe to be this: each segmental tube, before the formation of a glomerulus, gives off a hollow bud, towards the root of the mesentery, so that it becomes Y-shaped, the foot of the Y representing the peritoneal funnel, one limb being the suprarenal bud, and the other the connexion with the segmental duct. Almost immediately after its formation, the internal limb of the Y breaks off, and fuses with its fellows to form the unpaired rod at the root of the mesentery of which Balfour speaks.

The results of these observations we may sum up by saying that all

Vertebrates, except Amphioxus, have a portion of the kidney modified for some unknown purpose not connected with excretion; that in Cyclostomes the pronephros alone is so modified, in Teleostei the pro- and part of the meso-nephros; while in Elasmobranchs and the higher Vertebrates, the mesonephros alone gives rise to this organ, which has also, in these forms, acquired a secondary connexion with certain of the sympathetic ganglia.

IV. "The Pupil-Photometer." By J. Gorham, M.R.C.S. Eng. Communicated by Dr. Wilks, F.R.S. Received November 18, 1884.

(Abstract.)

It is agreed on all hands that the pupil of the eye owes its size to the quality of the light; contracting and dilating according to the intensities of the light. It is not agreed, however, if indeed it has ever become the subject of debate, that *its magnitudes may be reciprocally the measures of those intensities.* There are cogent reasons for believing that they are so, and hence that an instrument which measures the pupil's size, measures at the same time the light's intensity. The photometer was originally constructed for the former purpose only, and indicates the diameter of the pupil in hundredths of an inch. The diameter is found by directing the instrument applied to one eye, with both eyes open, towards a sheet of white paper, or the sky; the lid of the instrument is now revolved slowly until the two white disks *just touch one another at their edges.* The decimal fraction opposite the two apertures seen on the scale outside, indicates the diameter of the pupil in hundredths of an inch.

On examining hourly for several consecutive weeks the light of the day reflected from a given small area of the sky, certain recurring periodicities were observed in the pupil's magnitudes, and these are found to coincide with analogous alterations in the light's intensity; hence it was inferred that if the pupil owes its size to the intensity of the light, it became from that very fact a measure of that intensity. It is the object of the paper to substantiate this by experiment. To use the instrument for testing light of different intensities we first *set* the pupil to a light of a given intensity by using a Sugg's standard candle. This is placed at a distance of *one foot* from the eye, with a white surface close behind it, in a darkened room. The diameter of the pupil is now taken under the stimulus of the candle flame, and its measure is read off on the scale of the instrument. My own pupil, when impressed with such a light, measures invariably the 0·15 inch. We now place four such candles at *two feet* from the eye, when the

pupil will be found to remain stationary at the same magnitude as before. These results are in strict accordance with the rule that the intensity of the illumination of any body, in the presence of a source of light, will depend upon its distance from that source, and obeys the general law of radiant forces, *the intensity of the light varying inversely as the square of the distance of the luminous body.* Hence, if a single candle illuminates a body at one foot, four candles at two feet are required to produce equal illumination.

It will be observed that the process of equalizing the illumination becomes at the same time the measure of the number of candles used in the experiment. Hence, by an extension of the same principle, the candle-power of *any* luminous body may be found. Thus, if one candle at one foot is equal in intensity to four candles at two feet, it will be similarly equal to nine candles at three feet; where the number of candles in each case is found by taking the square of the distance. Let it be required, for instance, to take the candle-power of a gas-flame. Suppose now that the distance required to equalize the illuminations is equal to six feet, then the square of six being equal to thirty-six, the brilliancy of the flame will be equal to that of thirty-six candles.

The fact of the pupil varying in different individuals under a light of the same intensity, scarcely militates against the use of the instrument, for so far as observations extend it has been found that the pupil in each person has a range of its own, and that while the absolute numbers vary the ratios are the same. (See Diagram in paper.)

In taking observations we are not conscious of exercising any voluntary efforts of our own with a view to controlling the pupil. Its movements, on the contrary, are involuntary and *instinctive.* Herein, doubtless, consists the certainty with which they are performed.

December 1, 1884.

ANNIVERSARY MEETING.

THE TREASURER in the Chair.

The Report of the Auditors of the Treasurer's Accounts on the part of the Society was presented, by which it appears that the total receipts during the past year amount to £6,147 14*s.* 2*d.*, and that the total expenditure in the same period amounts to £5,456 7*s.* 10*d.*, leaving a balance of £691 6*s.* 4*d.* at the Bankers', and £22 19*s.* 9*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

Balfour, John Hutton, M.D., F.L.S.
Bentham, George, F.L.S.
Buccleuch, Walter Francis Montagu Douglas-Scott, Duke of, K.G., D.C.L.
Fawcett, Right Hon. Henry, M.A.
Frere, Right Hon. Sir Henry Bartle Edward, G.C.B., G.C.S.I.
Godwin-Austen, Robert Alfred Cloyne, F.G.S.
Hawkins, Cæsar Henry, F.R.C.S.
Manby, Charles, F.G.S.
Merrifield, Charles Watkins.
Rennie, James.
Smith, Robert Angus, Ph.D.
Thomson, Allen, M.D., D.C.L.
Todhunter, Isaac, M.A.
Townsend, Rev. Richard, M.A.
Tweedie, Alexander, M.D.
Watts, Henry, B.A.
Wilson, Sir Erasmus, Knt., F.R.C.S.
Wright, Thomas, M.D., F.G.S.

On the Foreign List.

Dumas, Jean Baptiste.
Kolbe, Adolph Wilhelm Hermann.
Würtz, Karl Adolph.

Withdrawn.

Carnarvon, Henry Howard Molyneux Herbert, Earl of.

Defaulters.

Bateman, James, M.A.
Clarke, Alex. Ross, Col. R.E.

Fellows elected since the last Anniversary.

Allman, Prof. George Johnston, LL.D.
Balfour, Prof. Isaac Bayley, D.Sc.
Baxendell, Joseph, F.R.A.S.
Bell, James, F.I.C.
Fry, Right Hon. Lord Justice Sir Edward.
Hartley, Prof. Walter Noel, F.R.S.E.
Herschel, Prof. Alexander Stewart, M.A.
Hudleston, Wilfrid H., M.A., F.G.S.
Lamb, Prof. Horace, M.A.
McKendrick, Prof. John G., M.D.
Ransome, Arthur, M.D.
Roy, Prof. Charles Smart, M.D.
Rücker, Prof. Arthur William, M.A.
Thomson, Joseph John, B.A.
Warren, Sir Charles, Colonel R.E., C.M.G.
Watson, Prof. Morrison, M.D.

On the Foreign List.

De Bary, Anton.
Gegenbaur, Carl.
Kronecker, Leopold.
Virchow, Rudolph.
Wiedemann, Gustav.

The Treasurer then addressed the Society as follows:—

The absence of our President from his post to-day must of necessity cast a certain amount of gloom over the proceedings at this our anniversary meeting, and, personally, I feel additional regret that it devolves upon me, as your Senior Vice-President and Treasurer, to be the unworthy occupant of the Chair on the present occasion. My regret at the absence of the President is, however, in one respect tempered by a strong hope, in which, I am sure, that you will all fervently join, that the timely retirement from arduous work which has been enforced upon him by his medical advisers may produce those beneficial results which we all so cordially desire, and that we may ere long again see among us our accomplished and valued President in renewed health and strength.

I must, however, turn from the expression of our hopes for the future, to that of our regrets for the past, and for a short time dwell upon the mournful list of vacancies which the ever-active hand of death has caused in our ranks during the past twelve months. In one respect only can a topic of consolation be found in this list. It is that in extent it is less than that of last year, the total number of our deceased Fellows being only eighteen on the home list and three on the foreign list, while those numbers were twenty-one and two respectively at our last anniversary.

All three foreign members whose loss we have on the present occasion to deplore were men of the highest distinction in chemical

science. Two were residents at Paris, both of whom had Chairs in the French Academy, of which the one had been since 1868 one of the Permanent Secretaries. I cannot dwell upon the discoveries and the remarkable career of M. Jean Baptiste André Dumas, whose energy and perspicuity even when past the limit of fourscore years, all those who of late have had the opportunity of being present at a meeting of the French Academy must have found reason to admire. An appreciative memoir of him by one of our own Fellows, who of all men living is perhaps the best qualified to judge of the value of his labours—Professor Hofmann—written while Dumas was still among us, will be found in the pages of "Nature,"* and a biographical notice by the same hand has appeared in our own "Proceedings." It will give some slight idea of the extent of time over which the labours of M. Dumas have extended if I mention that, so long ago as in 1843, he received the Copley Medal at our hands, at a time when his chemical and physiological researches had already extended over a period of twenty-two years. M. Dumas died at Cannes on the 11th of April last, and among the most touching of the speeches at his obsequies was that of M. Würtz, whose own decease took place on the 12th of the following month.

Although nearly twenty years younger than M. Dumas, M. Karl Adolph Würtz had for a long time been one of the most distinguished leaders of modern chemical science, especially in the department of organic chemistry, and his merits were recognised by this Society in 1864, when he was elected one of our foreign members, and again in 1881, when the Copley Medal was awarded to him.

The third chemist on the foreign list whom we have lost is our Davy Medallist, Professor Kolbe, whose sudden death took place only on Wednesday last, and of whose merits I shall have to say a few words later on.

Among our English Fellows was a contemporary of Würtz, who, like him, had been a pupil of the illustrious Liebig, but whose bent was more on the practical than on the theoretical side of chemistry—I mean Dr. Angus Smith, whose official labours in favour of pure air and pure water combined both tact and zeal, and were productive of highly beneficial results.

One other chemist has been taken from among us, Mr. Henry Watts, the well-known editor of the "Dictionary of Chemistry," and of more than one issue of "Fownes's Manual."

Our other losses extend over various departments of science. In botany our ranks are thinned by the death of Dr. John Hutton Balfour, formerly Professor of Botany at Glasgow and the Emeritus Professor of that Chair in the University of Edinburgh; and of the

* "Proc. Roy. Soc.," vol. xxxviii, x. "Nature,' vol. xxi, February 6, 1880.

veteran Mr. George Bentham, who had nearly completed his eighty-fourth year. During his long and varied experiences of life, botany was his constant pursuit and study; and some thirty years ago, after presenting his fine collections and library to the Royal Gardens at Kew, he devoted himself to labouring there on the Floras of Hong Kong and Australia, and, in conjunction with Sir Joseph Hooker, on the "Genera Plantarum," until his health gave way in the spring of last year. The exceptional value of his botanical work was recognised by this Society in 1859, when a Royal Medal was awarded to him, and his regard for the Society has been testified by his making a bequest of 1,000*l.* to our Scientific Relief Fund.

Among mathematicians we have lost Dr. Isaac Todhunter, whose educational treatises have for many years been recognised as standard works, and whose elaborate histories of different branches of mathematical science have earned for him a high reputation; and Mr. Charles W. Merrifield, who, in addition to achieving distinction by his educational works on arithmetic and mathematics, did much in the direction of the practical application of science, and at the Royal School of Naval Architecture and Marine Engineering successfully laboured in improving the stability and the sea-going powers of the British Navy.

Another distinguished mathematician whom we have within the last few weeks had the misfortune to lose, was the Rev. Richard Townsend, Professor of Natural Philosophy in the University of Dublin, whose labours in the more abstruse fields of geometrical speculation extended over a period of nearly forty years. Mr. James Rennie was also a votary of mathematical research.

Among practical men of science, the veteran Mr. Charles Manby, who for forty-five years had been Secretary or Honorary Secretary of the Institution of Civil Engineers, will deservedly take a high place.

The anatomical and physiological labours of Professor Allen Thomson had extended over the longer term of fifty-four years, and few possessed the power of clearer exposition than he, while for acts of personal kindness there must be many besides myself who owe him a deep debt of gratitude.

Among others connected with the medical profession we miss the distinguished surgeon Mr. Cæsar Hawkins, Dr. Alexander Tweedie, and Sir Erasmus Wilson, whose name will long survive, not only in connexion with dermatology and the Chair of Pathological Anatomy at Aberdeen, but with the Egyptian obelisk, known as Cleopatra's Needle, the presence of which in London is entirely due to his liberality.

In Mr. R. A. C. Godwin-Austen we have lost one who for nearly fifty years had ranked among the foremost of English geologists. His manifold observations will be recorded elsewhere, but as an instance of his critical powers, I may mention his now classical paper on the

possible extension of the Coal Measures beneath the south-eastern part of England, read in 1855, his speculations in which as to the western extension of the axis of Artois, all recent deep borings within the Thames basin have so fully substantiated.

In Dr. Wright we have lost an accomplished palæontologist whose knowledge of the fossil echinodermata and ammonitidæ was almost unrivalled.

The Duke of Buccleuch had for fifty years been one of our Fellows, and in 1867 occupied the position of President of the British Association; while Sir Bartle Frere, although an ethnologist and geographer, will probably be better known as an able and energetic public servant and administrator than as a man of science.

In common with the nation at large, we have to deplore the untimely and unexpected decease of another distinguished statesman, the late Postmaster-General, Mr. Fawcett. A man of rare mental powers, the effect upon him of the greatest of all physical deprivations, the loss of sight, was only to make him rise superior to his misfortune. As a student of political economy he attained a high reputation, and he turned his mastery of the subject to good account when he entered into the sphere of public life, towards which his natural aspirations led him. His singleness and honesty of purpose, the inborn justice of his well-balanced mind, his devotion to the public good, and his invariable courtesy, endeared him alike to political friends and opponents; while to those who were brought into more immediate contact with him, his truly sympathetic nature and the marvellous memory, which preserved even minute details of former conversations, gave a charm to his intercourse which none who enjoyed it will ever forget.

As I have already observed, our losses on the home list, including one resignation of Fellowship and one removal from our list, are less than in many former years, being altogether twenty-one in number; we have, on the other hand, elected sixteen Fellows, including one Privy Councillor. It would, however, appear that our numbers are gradually attaining to something like a state of equilibrium, and that if our elections continue to be limited as at present, the roll of the Society will remain at its present standard of about 470 Fellows. Looking at the recognised longevity of scientific men and the age at which many now achieve the distinction of being elected into the Society, it seems to me not improbable that our numbers will ere long show a tendency to increase rather than to diminish.

Of the "Philosophical Transactions," three parts, and of the "Proceedings," eleven numbers, have been published; while the number of papers received during the past year was 100, as compared with 103 in the previous year. Of these the most numerous have been in the departments of electricity and magnetism, though

physics and mathematics, chemistry, mineralogy, anatomy and physiology, botany, and morphology have all had their share.

It is hardly within my province to select any papers that we have published as being the most worthy of mention. The mere fact that they have appeared in the "Philosophical Transactions" is a sufficient voucher for their value. I may, however, call attention to the report of Captain Abney and Dr. Schuster, our Bakerian lecturer for the present year, on the total Solar Eclipse of May 17, 1882, which is the outcome of an expedition, towards which a grant of 350*l.* was made from our Donation Fund. Some of the results were mentioned by Mr. Spottiswoode in his Presidential Address of 1882, but the value of the details with regard to the corona, and the success which attended the efforts of the photographers, can only be estimated from an examination of the paper itself. The detailed results obtained by the photographers who accompanied the American expedition to Caroline Island in the South Pacific in order to observe the Solar Eclipse of the 5th of May, 1883, have not yet been brought before the Society.

In respect to biological studies, our record of the past year, though it does not contain the announcement of any very startling results, gives evidence of fruitful activity along various lines of research.

In Botany, Mr. Gardiner has continued his observations on the important subject of the continuity of protoplasm in vegetable cells, which was referred to in the President's Address of last year; he has also brought forward some interesting results derived from an examination of the changes in the gland-cells of Dionæa, which serve still further to illustrate the identity of the fundamental physiological processes in plants and animals. Mr. Bower has dealt with the morphology of the leaf in certain plants, in a memoir both valuable in itself, and noteworthy because hitherto the study of abstract vegetable morphology has perhaps not obtained in this country the attention which it deserves, and which has been given to it in other countries, especially in Germany.

In Physiology two important papers have been presented on the difficult subject of the functions of the cerebral convolutions, one by Drs. Ferrier and Yeo, and the other by Professor Schäfer and Mr. Horsley. Both contain observations which demand careful consideration by all physiologists.

The results of the study of animal forms which is happily being carried on with great activity, I may say, all over the United Kingdom, are for various reasons principally recorded elsewhere than in the pages of the "Transactions" or "Proceedings" of this Society. Nevertheless, this subject has also been fairly represented at our meetings. Our distinguished and unwearied Fellow, Professor W. Kitchen Parker, is still continuing his elaborate and valuable researches on the vertebrate skull, and during the past session the Society has

had the pleasure of receiving several short papers, expounded in person by their author, from a veteran in the study of animal morphology, whose first communication to the Society bears the date of 1832. I need hardly say I mean Sir Richard Owen.

A few words must be said with regard to the acquisitions made by our library and collections. Our gallery of portraits has, through the kind liberality of Dr. Wilson, of Florence, received two important additions in the form of half-length original portraits of the distinguished mathematicians and philosophers, Leibnitz and Viviani, both of whom were Foreign Members of this Society. When we remember the warmth of feeling with which the rival claims of Newton and Leibnitz to the invention of Fluxions or the Differential Calculus were for many years discussed, and call to mind that the question occupied the attention of a Committee of this Society in 1712, which reported in favour of Newton's claims, we may rejoice that the heat of the controversy is long since over, and congratulate ourselves that the portraits of these rival intellectual giants now hang peacefully side by side on our walls. The portrait of Viviani, the great geometrician, the pupil of Galileo and the associate of Torricelli, and a contemporary of Newton and Leibnitz, finds also a fitting resting-place in our gallery.

Our portfolio of engraved and lithographic portraits of scientific men has been considerably augmented by liberal donations from the executors of our former President, the late Sir Edward Sabine, through Mr. R. H. Scott.

The Lalande medal, which had been awarded by the French Academy to Sir Edward in 1826, and which, together with a Royal Gold Medal, was presented to the Scientific Relief Fund, was by the Council redeemed from the Fund, and will be preserved among our other medals as a memorial of one who for so long a period rendered important services to the Society. A bronze medal of our distinguished Fellow, Professor Sylvester, has been presented to our collection by the Johns Hopkins University, at Baltimore.

The library itself has during the past year received by donations about 380 complete volumes, besides about 240 pamphlets, and more than 2400 parts of serials; 24 charts have also been presented to it.

With regard to our finances, I may venture to say, as your Treasurer, that I consider them to be in a satisfactory condition.

I must now turn to some of the subjects which, during the past year, have occupied the attention of the President and Council, and which in more than one instance have brought them into communication with Her Majesty's Government.

In July of last year a letter from the Treasury was received requesting our opinion as to the desirability of subsidising the Armagh Observatory, the income of which had been materially reduced, owing to recent legislation in Ireland. In reply to this an answer

was sent pointing out the good work that had been done in the Observatory, and the exceptional character of the institution, and recommending it to the favourable consideration of the Government. Unfortunately, however, the loss of income applicable to the maintenance of an observatory has not been made good, though the Treasury, "having regard to the advice of the Royal Society, and to the diminution in the income of the Observatory," has granted a sum of 2,000*l.* in aid of its funds, the annual income derived from which sum is to be applied by trustees to the maintenance and purchase of instruments and apparatus.

Another correspondence with the Treasury as to the bathymetrical survey of the lakes within the British Isles did not lead to any concession in favour of such a necessary complement to the National Ordnance Survey, though the omission in our maps of all details relating to the depth of our lakes and the contour of their beds, cannot be justified on practical, and much less on scientific grounds.

In May last the Astronomer Royal brought under our notice the position of this country with respect to the International Bureau des Poids et Mesures, an institution established by what is commonly known as the Metric Convention; and it was resolved that in the opinion of the President and Council it is highly desirable that our country should take part in the International Commission of Weights and Measures, and contribute the sum which our adhesion would entail. A deputation was appointed to bring the subject under the notice of the Lords of the Treasury, and after some correspondence, the Society was authorised to enter into informal negotiations with the officers of the Bureau, with the happy result that Great Britain was invited to join the Metric Convention, and through her Ambassador at Paris has, I believe, now given in her adhesion to it, and is entitled to all the privileges accorded by the Bureau. The appliances at the command of the Bureau for the verification not only of the standards of the metric system, but of other units of measure, far surpass in scientific accuracy anything that is at present available in this country, and we now enjoy the double advantage of being removed from the state of isolation in which for some years we have stood in regard to the other nations of Europe, and of now being affiliated to an establishment in which the verification of standards has been carried to the highest perfection. At the same time it is distinctly understood that our adhesion to the Bureau in no way commits the Government of this country to any change of opinion favourable to the adoption of the metric system, but that our entire freedom to retain our own system of weights and measures is absolutely preserved. Whatever may be the advantages of the metric system from a scientific point of view, the question whether a scale of weights, money, and measures, which in its lowest denominations follows a duodecimal

rather than a decimal system, is not better adapted for the convenience of daily life, is one that by many is regarded as fairly open to discussion.

Another event of both scientific and national importance has been the meeting of an International Conference on the subject of a Prime Meridian of Longitude. The desirability of a common starting-point from which to reckon degrees of longitude has long been felt among all civilised nations, especially those of a maritime character, and was discussed at some length during the Congress of the International Geodetic Association at Rome in October, 1883. It was not, however, until the end of last year that invitations were issued by the United States Government for different countries to send representatives to an International Conference to be held in the city of Washington, for the purpose of discussing and, if possible, fixing upon a meridian proper to be employed as a common zero of longitude and standard of time-reckoning throughout the globe. The letter of invitation addressed to this country was referred to the President and Council of this Society with a request to advise the Government whether it was desirable in the interests of science to accept the invitation. In reply an opinion was expressed as to the high importance both for the interests of science in general, and of our own country in particular, that our Government should be represented at the Conference, and the Treasury at once sanctioned the expense of sending two delegates to Washington. These were Sir Frederick Evans, the late Hydrographer to the Admiralty, and Professor J. C. Adams. General Strachey, the Chairman of the Meteorological Committee, was also nominated to represent India, and Mr. Fleming to represent the Dominion of Canada. The delegates assembled at Washington in the month of October last, and proceeded to discuss the questions whether a single prime meridian for all countries could be adopted, and if so, through what point on the earth's surface should that meridian be drawn. After long discussion it was eventually resolved that the meridian of Greenwich should be generally adopted, twenty-two* of the nations voting in favour of this measure, and only one, San Domingo, against it. The representatives of France and Brazil abstained from voting. The proposal for the adoption of Greenwich was made by one of the representatives of the United States of America, and was fully discussed. Our own representatives ably supported the proposal, and another of our most distinguished Fellows, Sir William Thomson, who happened to be in America at the time, was courteously invited to attend the meetings of the Conference, and on the request of the President to express

* The following nations voted in favour of Greenwich :—Austria, Chili, Colombia, Costa Rica, Great Britain, Guatemala, Hawaii, Italy, Japan, Liberia, Mexico, Netherlands, Paraguay, Russia, Salvador, Spain, Sweden, Switzerland, Turkey, United States, and Venezuela.

his opinions. The arguments adduced in favour of the adoption of Greenwich were such as must commend themselves to all unprejudiced minds. It could hardly be expected that there should be any special spot upon the earth's surface from which longitude would naturally be reckoned, and the whole question, apart from any sentimental or patriotic feelings, is therefore one of the greatest convenience. Were the employment of degrees of longitude as general geographical units entirely unheard of up to the present time, it would, of course, be a matter of perfect indifference whether the datum was at Greenwich, Paris, the Ferroe Isles, or any other spot. The meridians most in use are those of the two former places, and when we come to consider that, as was pointed out, the shipping tonnage controlled by the Greenwich standard of longitude is about 14 million tons, while that controlled by the longitude of Paris amounts to $1\frac{3}{4}$ million tons only, the preponderance of convenience in favour of the former is placed beyond all dispute. The use of nautical charts constructed from the meridian of Greenwich, and also of the Greenwich Nautical Almanack, is by no means confined to the British Navy, for numerous other nations have availed themselves of the long-extended labours of our hydrographers, and the computations of our astronomers. At the same time there is no one among us who would for a moment venture to dispute the vast services to science which have been rendered by French astronomers and geographers, nor should we contest the right of French *savants* to regard Paris as the *μεσόμφαλος ἑστία* of all other branches of science; the question of a common zero of longitude, however, is not only of scientific but of commercial impórtance, and we may be confident that eventually our friends on the other side of the Channel, whose metric system has been so largely adopted by other countries, will in their turn sacrifice their own meridian, and adopt that which all neighbouring countries have declared to be the most convenient for general use. If some French locality on the meridian of Greenwich, such for instance as Argentan, were nominally the French datum, the results would be the same so far as maps and charts are concerned, and the natural patriotism of the French nation would remain uninjured.

The adoption of an universal day has also been recommended by the Conference. It is to be a mean solar day to begin for all the world at the moment of mean midnight of the initial meridian, coinciding with the beginning of the civil day and date of that meridian, and is to be counted from zero up to twenty-four hours.

The great volcanic eruption of Krakatoa, in the Straits of Sunda, which took place in August of last year, was followed by remarkable atmospheric and other disturbances, observations on which have been communicated to this and various other learned Societies, and have led to much interesting discussion. The fact, as pointed out by

General Strachey and Mr. Scott, that at some barometrical stations the atmospheric wave caused by the eruption was still to be traced until about 122 hours after its origin, and that it must have travelled more than three times round the entire circuit of the earth, shows how vast must have been the initial disturbance causing the wave. The possibility of the remarkable atmospheric appearances which so constantly accompanied the rising and setting of the sun for some months subsequent to the eruption, being due to volcanic dust in suspension in the air, offered a farther incentive to investigate the whole history of the eruption. In consequence the Council in January last nominated a Committee to collect the various accounts of the volcanic eruption at Krakatoa and attendant phenomena, in such form as shall best provide for their preservation and promote their usefulness, and a sum of 100*l.* in all has been granted from the Donation Fund to defray the expenses of the Committee. A Committee of the Royal Meteorological Society, which had already been appointed to study the sunset phenomena of 1883–84, joined forces with our Committee, and their united labours, with Mr. A. Ramsay as secretary, have resulted in the accumulation of a voluminous mass of material. The accounts given in the chief British and foreign scientific serials have been extracted and classified, and the times of the various observations reduced to Greenwich mean time.

The literature on the subject, as Mr. Symons informs me, seems almost inexhaustible, and the Committee, feeling that some limit must be adopted, have now stopped the collection of further data, and are engaged in the discussion of what have already been obtained. The MS. is classified according to subjects, and each of these is being studied by the members of the Committee most familiar with it. It is to be hoped that in the ensuing session we shall be favoured with some of the results of their labours.

In the Presidential Address of last year mention was made of a series of borings which it was proposed to make across the delta of the Nile in Egypt, and which, with the sanction of the Secretary of State for War, had been entrusted to the officer commanding the Royal Engineers attached to the army of occupation in Egypt. Shortly afterwards a Report from Colonel Heriot Maitland, R.E., and Major R. H. Williams, R.E., was received, giving an account of a boring at Kasr-el-Nil, near Cairo, which had been carried to a depth of 45 feet, and of a second boring at Kafr Zaiyat, where a depth of 84 feet was attained. In both cases great difficulties had to be surmounted, but in neither was the solid rock reached beneath the superficial deposits. A second Report from the same officers, dated January 18th last, states that a third boring had been executed at Tantah, this time by the sappers of the Royal Engineers, and not by Arab workmen, though still with but imperfect tools. In this instance a depth of 73 feet was reached, but

again without finding the solid rock. Samples of the materials obtained at different depths in these three borings have been forwarded to the Society by the War Department, and Professor Judd has kindly undertaken their microscopic examination, and will shortly report the results of his labours to the Committee in charge of the subject.

With regard to the continuance of the borings, which seem to promise information of great value and interest, it is to be feared that the attention of the military authorities will for some time to come be attracted to more urgent business, though the Council of this Society has expressed its willingness to grant from the Donation Fund a further sum of 200*l.*, with the view of obtaining better apparatus for boring than that which has hitherto been employed.

The publication of the results of the "Challenger" Expedition, with which a Committee of this Society is to some extent concerned, has made considerable progress during the past year. Mr. Murray informs me that 47 Reports, forming 13 large quarto volumes, with 6276 pages of letter-press, 1051 lithographic plates, many woodcuts, charts, and other illustrations, have now been published. Nine other Reports are now being printed, and the eleventh Zoological volume and the first Botanical volume will be issued during the present financial year.

The work connected with the remaining thirty-six memoirs is making satisfactory progress, a large instalment of the manuscript being already prepared, and many of the plates either already printed off or drawn on the stone.

There has been an unavoidable delay in the case of the two volumes containing the narrative of the cruise, and a general account of the scientific results of the Expedition, but it is expected that they will be issued within the next three months, and possibly before the end of the current year.

It was estimated that the investigations connected with the collections and observations made during the Expedition would be completed and published in 1887, and Mr. Murray has every reason to believe that the work will be finished within the estimated time.

The tenth Zoological volume which has just been issued, contains important Reports on the Nudibranchiata, Myzostomida, and Cirripedia, by Drs. Rudolph Berg, L. von Graff, and P. P. C. Stock, as well as on the Cheilostomata, a sub-order of the Polyzoa, by Mr. George Busk. A first instalment of the Anthropological Report is also given by Professor William Turner, in a detailed examination of the human crania, upwards of 60 in number, brought home by the Expedition. The total number of crania, however, described and tabulated in the memoir is 143, the whole from aboriginal and as yet uncivilised people. The previous Zoological volume is devoted to an exhaustive examination of the Foraminifera, by Mr. H. B. Brady.

The subject of the International Polar Observations, which were carried out during the twelve months ending with August, 1883, has been touched on in recent Presidential Addresses, and in that for 1881 the general outline of the whole scheme was indicated. Now, however, the programme then only sketched out has been more than fulfilled, no less than 14 stations for observers, 12 for the Northern and 2 for the Southern Hemisphere, having been organised. Of all the expeditions, one only, that from Holland, failed to reach its destination, Dickson Harbour, at the mouth of the Obi river, as it was beset by ice in the Kara Sea, in the month of September, 1882. The ship which carried the members of the expedition sank in the month of July, 1883, but they all reached home in safety, having carried out their observations as fully as lay in their power. One of the two expeditions sent out by the Chief Signal Office, Washington, was not so fortunate. The party under Lieutenant Greely, after spending over two years at Lady Franklin Bay, Smith's Sound, was eventually rescued at Cape Sabine, in July last, but not before many of its members had succumbed beneath the fearful hardships of their protracted Arctic sojourn.

The actual points of observation, going eastwards from Behring's Straits, and the States, which sent out the expedition, are tabulated below :—

Point Barrow............	The United States.
Fort Rae................	Great Britain and Canada.
Lady Franklin Bay	The United States.
Cumberland Sound	Germany.
Godthaab	Denmark.
Jan Mayen	Austria.
Spitzbergen..............	Sweden.
Bossekop................	Norway.
Sodankylä	Finland.
Nova Zembla	Russia.
Mouth of the Lena........	Russia.
The Kara Sea............	Holland.

In the Southern Hemisphere—

Cape Horn	France.
South Georgia	Germany.

At all of these stations observations were carried on for a year, and at some for even a longer period.

In the month of April last a conference was held at Vienna, to decide as to the form and mode of discussion and publication of the results, and it may be hoped that these will appear before the end of 1885.

Of the serial publication, "Communications from the International Polar Commission," six parts, with an aggregate of 334 pages, have already appeared, and in it will be found all particulars of the undertaking.

The regulations under which the Government Grant of 4,000*l.* is administered have during the past year been again under discussion, and have in some respects been slightly modified. It is, of course, needless to repeat that this grant, though nominally made to the Royal Society, in no way adds to its funds, while its administration rests with a Committee of from sixty or seventy members, many of whom are not of necessity Fellows of our Society. As the grant is now made in two instalments, it has been arranged that the meetings of the Committee shall be held twice in each year, viz., in May and December, which it is hoped will amply meet the convenience of applicants for grants.

In looking back upon the grants which have been made during the past year, I think that a tendency may be observed on the part of the Committee to devote considerable sums in aid of extensive researches rather than to fritter away the money at their disposal in a series of small grants. They have, for instance, allotted the sum of 500*l.* towards the exploration of Kilimandjáro and the adjoining mountains of Tropical Africa, and 200*l.* in aid of an expedition for the exploration of the mountain of Roraima in British Guiana. A grant of 200*l.* has also been made towards a report on the Flora of China; while 300*l.* has been allotted towards the extra accommodation and instruments for magnetic observations in the new Observatory of the Royal Cornwall Polytechnic Society. It will be remembered that, in his Address last year, the President called attention to the discovery by Dr. Huggins of a method of photographing the solar corona without an eclipse; and, for the purpose of making further experiments in this direction, and for carrying on other physical observations at some place of high elevation and of easy access, a grant of 250*l.* was placed at the disposal of a Committee. The place of observation selected by the Committee was the Riffel, near Zermatt, in Switzerland, which has an elevation of 8500 feet, and possesses important advantages both of access, and of hotel accommodation. They appointed Mr. C. Ray Woods, who had had experience in photographing the corona during the eclipse of 1882 in Egypt, and again in Caroline Island in 1883, to take charge of the work under the instructions of Dr. Huggins and Captain Abney.

Mr. Woods arrived at the Riffel in the beginning of July, when he erected the necessary instruments under a tent of "Willesdenized" paper, and continued at work there until the 21st of September. Unfortunately, the present year has been exceptionally unfavourable for work on the corona, in consequence of an unusual want of trans-

parency in the higher regions of the atmosphere. This probably may be owing to the presence there of ice-crystals or of small particles of matter of some kind, such as, personally, I am tempted to think, might be due to the Krakatoa eruption. Whatever the cause, the sky as seen from the Riffel was far from being so clear as it has been during former years. Mr. Woods observed that the freer the lower air was from cloud and mist, the more distinctly came out a great aureola around the sun, which he found to have a diameter of about 44°, and to be of a faint red near the outer boundary, and bluish-white within, up to the sun's limb.

These unfavourable conditions of the atmosphere have made it impossible for Dr. Huggins to obtain any photographs of the corona in England. The great advantage at the Riffel of being free from the light scattered from the lower 8000 feet of air has enabled Mr. Woods, notwithstanding the serious drawback of the persistent aureola, to obtain about 150 photographs, of which more than half are sufficiently good to show the general form of the corona, and a smaller number, the stronger details of that part of it which lies within from 8′ to 12′ of the sun's limb. It would be premature to express any opinion as to the information which may eventually come out from the Riffel plates. They are now being drawn preparatory to a full discussion. In the meantime I may congratulate the Society upon the confirmation of the hope expressed by our President at the last anniversary, "that a new and powerful method of investigation has been placed in the hands of students of solar physics."

Another of the grants made by the Committee has also contributed to important scientific results, as it has enabled Mr. Caldwell to make some important observations on the early stages of the monotreme ovum, a brief account of which was communicated to the meeting of the British Association for the Advancement of Science at Montreal. A fuller account of the observations, such as is necessary for the adequate appreciation of their importance and bearings, will, I hope, be laid before the Society during the ensuing session, when we shall also probably hear the result of similar investigations in like manner rendered possible by the existence of the Government Grant.

Some slight aid has been rendered from the same source towards the reduction of observations carried on at the meteorological station on the summit of Ben Nevis. This Observatory, situated on the highest point within the United Kingdom, has through the past year been under the charge of Mr. R. T. Osmond and two assistants. During the summer months the buildings of the Observatory have been enlarged by the addition of new observing-rooms and increased accommodation for the observers and any scientific workers who may desire to carry on those physical researches for which the climate and position of Ben Nevis afford many facilities.

The erection and equipment of the Observatory have cost more than 5,000*l.*; and, in connexion with the observations carried on at the top of the mountain, others have been daily made near the sea-level at Fort William. A first report on these conjoint high and low level observations, which began in 1881, has been prepared by Mr. Buchan.* The monthly normals for atmospheric pressure and temperature have been approximately determined for the Observatory. Important results have also been obtained relating to the decrement of temperature with height, for different months of the year and hours of the day, the diurnal variations of the wind's velocity, the very large increase in the rainfall on and near the summit, and the altogether unexpected hygrometric conditions of the air in their relation to the cyclones and anti-cyclones of north-western Europe.

Another of the funds at our disposal, the Scientific Relief Fund, requires a few words of mention. Its resources have been considerably enriched during the past year by the legacy of 1,000*l.* from Sir William Siemens, and nearly 50*l.* from the medals offered by the executors of the late Sir E. Sabine; and the legacy of 1,000*l.* from the late Mr. Bentham will, it is hoped, ere long be received; but even with these munificent additions the income of the fund will amount to only 250*l.* per annum, while last year the calls upon it amounted, I regret to say, to no less than 450*l.* The incalculable value of such a fund to men of science or their families requiring temporary aid must be apparent to all, and looking at the unfortunate necessity for its existence which the calls upon it prove, I venture to commend it to your support. It will, perhaps, not be out of place here to say a few words with regard to the administration of this fund, the existence of which dates from 1859, and is in a great degree due to the exertions of the late Mr. Gassiot. The Council of the Royal Society takes charge of any sums contributed to the fund and invests them, applying the interest in grants for the relief of such scientific men or their families as may from time to time require or deserve assistance. These grants are, however, made only on the recommendation of a committee of seven members who investigate the cases before them, and applications for relief cannot be entertained except on the recommendation of the President of one of the following chartered societies, the Astronomical, Chemical, Geographical, Geological, Linnean, Royal, and Zoological Societies. Since January, 1861, when the first grant was made, the total number of grants is eighty-eight, and the total sum distributed 4,340*l.*

Our Donation Fund has also proved of much service, and several of the applications for comparatively small amounts, which were referred by the Government Grant Committee for the consideration of the Council of the Royal Society, were met by grants from this source.

* "Journ. of the Scottish Meteorol. Soc.," 3rd Series, No. 1 (1884), p. 4.

This most valuable fund, the annual income of which is now about 400*l*., has, during the past year, rendered important aid to various scientific objects. From it considerable grants have been made towards obtaining specimens of Hatteria and Apteryx; for expenses incurred on account of the voyage and investigations of the surveying ship "Triton;" for collection of materials relating to the Krakatoa eruption; towards the borings in the Delta of Egypt; and, lastly, in aid of the Marine Biological Association.

The close connexion of the future of our fisheries with the advancement of certain branches of zoological science was commented upon by our President in his last anniversary address, and I have now to record the foundation of two establishments devoted to marine research. The first of these is the station established at Granton, near Edinburgh, mainly through the energetic labours of Mr. John Murray of the "Challenger" expedition. It consists of a floating laboratory where physical and biological investigations are carried on, and it is provided with a steam yacht for taking observations at sea and procuring specimens for examination. Chemical and other laboratories are now being erected on the shore, close to the enclosed piece of water where the floating laboratory is moored. Two naturalists, a chemist and a botanist, are permanently attached to the station, and have an engineer, a fisherman, and three attendants to assist them in conducting regular systematic observations. £2,500 have been spent on the equipment of the station, and it has at present an income of 400*l*. a-year, independent of the grants which some of the permanent staff have received from the Government Grant Committee to aid them in their researches. It is well that it should be known that any scientific observer is at liberty to make use of the station free of charge; indeed, during the past year five gentlemen and one lady have availed themselves of this privilege during short periods of time.

But the movement in favour of such stations has not been confined to Scotland, for I have also to chronicle the foundation of the Marine Biological Association, which originated in a meeting held in these rooms on March 31st last, our President being in the chair, and many of our principal naturalists taking part in the proceedings. The formation of such an Association has long been hoped for by many interested in obtaining a correct knowledge of the life and conditions of our sea-coast, who are now principally indebted to Professor Ray Lankester for the realisation of their hopes. The operations of the Association will in no way clash with those of the station at Granton, but both institutions will work towards a common end. One effect, indeed, of the new Association will probably be to render all the more fruitful the labours on the more northern shores by instituting similar researches at other parts of the coast of our island.

The work of the Association is as yet in the inceptive stage, but a site well adapted for a marine observatory will, through the liberal endeavours of the Mayor and Corporation of Plymouth, probably be secured in that town; some citizens of which have also promised a noble donation of 1,000*l.* towards its erection. The Clothworkers' Company has contributed 500*l.*, and the Mercers' Company 250 guineas, while the Council of this Society has also shown its sympathy with the movement by a grant of 250*l.*, and the British Association by one of 150*l.* Handsome donations have also been made by private individuals, and the number of members of the Association is gradually increasing. When once the station is completed and at work, and its aims and operations become better known, I make little doubt that it will receive a much larger share of public support. But before the station can be erected and in work, it is calculated that an outlay of 10,000*l.* is necessary for its building and equipment, of which as yet not quite half is forthcoming, and I venture to take this opportunity of enforcing the claims of the Association upon all who are interested in "improving natural knowledge." As has already been well pointed out in the memorandum issued by the Association, "great "scientific and practical results have been obtained in other countries, "notably in the United States of America, in Germany, France, and "Italy, by studies carried on through such laboratories as the Marine "Biological Association proposes to erect in this country," and I may add as that already at work at Granton. When we consider the enormous importance of our fisheries, and how large may be the amount of material benefit derived from a scientific investigation of the causes of their increase and diminution, it will, I think, be evident that the work to be carried on at these stations is not only for such a purpose as the development of abstract biological science, important as that may be, but for the advancement of our national resources. It is, therefore, to be hoped that in addition to the private support which they will receive, they may in some manner be recognised by the nation at large as centres for carrying out systematic investigations into the circumstances determining marine life, from which a portion of our food supply is drawn, and a much larger portion might probably be derived. The importance of our sea fisheries, which it will be one of the principal objects of the Association to promote, has of late years been more fully recognised, and the recent International Fisheries Exhibition has done much to popularise the subject; while the official appointment of our President also proves that in the opinion of our Government the scientific aspects of our fisheries are not to be neglected.

In the last Presidential Address reference was made to the great desirability of carrying out, on the part of this country, investigations into the nature of cholera in continuation and extension of

those so zealously and bravely initiated by the distinguished German inquirer Koch. Although the Society has had no very direct influence in the matter, the Fellows will, I venture to think, regard it as a subject for congratulation that the wish then expressed from this chair has been fulfilled, and that the distinguished expert in such questions—our Fellow, Dr. Klein—is at present engaged in India in the investigation of cholera at the instance of the Indian Government. It is sad to think how much nearer our own shores such investigations might have been conducted; may it be long ere they can be instituted on this side of the Channel.

These remarks have already extended to such a length that I can now only briefly refer to a few of the events of scientific interest which have during the past year occupied the attention of the Society or of a large number of its Fellows. In the month of April last the University of Edinburgh celebrated its tercentenary with great pomp and no less hospitality, upwards of 120 delegates from various universities and other learned bodies being invited as guests. On this occasion Lord Rayleigh kindly consented to be our representative, and was among those on whom the University conferred the honorary degree of LL.D. The same distinguished Fellow occupied the Presidential chair at the meeting of the British Association for the Advancement of Science at Montreal, on which occasion many of our body took the opportunity of crossing the Atlantic. Owing to the munificent liberality and ungrudging hospitality of our brethren in the Dominion of Canada, the somewhat bold experiment of holding a meeting of the Association beyond the limits of the British Isles has proved a great success, though, perhaps, it is an experiment which would require exceptional conditions to be successfully repeated.

The Society was represented by delegates at the meeting of the American Association for the Advancement of Science at Philadelphia in September last. The Electrical Exhibition at the same place resulted in the formation of a Memorial Library in connexion with the Franklin Institute, to which separate copies of the Papers relating to Electricity that have appeared in the "Philosophical Transactions" have been granted by the Council. An Electrical Congress at Paris, and an Ornithological one at Vienna have also been among the events of the year.

Subscribers to the Darwin Memorial Fund will be pleased to hear that a fine block of marble has been secured for the statue to be erected in the Natural History Museum at South Kensington, and I am glad to learn from Mr. Boehm that his work will probably be completed by the end of this year. When the total cost of the statue has been ascertained, it will be necessary to hold a meeting of the Committee in charge of the Memorial Fund to determine the manner in which the balance is to be applied.

It now only remains for me to thank the Fellows and others conversant with the subjects on which I have touched, for information kindly afforded me; to thank you for the attention with which you have listened to me, and to express a hope that it may not again for many years occur that the Anniversary Address from this Presidential chair shall have to be delivered by deputy.

The Vice-President in the Chair then proceeded to the presentation of the medals.

The Copley Medal has been awarded to Professor Carl Ludwig, of Leipzig, for his investigations in Physiology, and the great services which he has rendered to Physiological Science. During the last forty years, the advances that have been made both in the powers of the microscope and in the methods of exact physical and chemical observation have reacted in a remarkable manner on the development of physiological knowledge, and during nearly the whole of that long period the name of Carl Ludwig has been prominent on the list of investigators, and to the progress that marks that period he has probably contributed more than any man living. The determination of the exact share in this progress really due to himself alone is perhaps somewhat obscured by the generous way in which he has always placed his ideas and his knowledge at the service of those who have assisted in his laboratory, but there can be no doubt that a large proportion of our present knowledge of the phenomena of blood pressure and of the vaso-motor system, of the physiology of the heart, and of the spinal cord, and of digestion and nutrition, is due to him and to his numerous pupils.

Moreover, the very fact that he has allowed so many others to share in his experience and to become trained in his methods, would in itself entitle him to some mark of our gratitude and esteem.

A Royal Medal has been awarded to Professor George H. Darwin for his mathematical investigations on the effects of an imperfect rigidity of the earth, and on tides. The principal results of these researches have already been published in the "Philosophical Transactions," and are in the hands of the Fellows, who will no doubt rejoice to see the son of so distinguished a father still doing honour to the name of Darwin.

A Royal Medal has been awarded to Professor Daniel Oliver for his investigations in the classification of plants, and the services which he has rendered to taxonomic Botany. These services have been of the highest order; but apart from his numerous published papers, and his work on the Flora of Tropical Africa, his fertile labours in the Kew Herbarium would alone entitle him to recognition at the

hands of this Society. When it is borne in mind that the labours of Sir Joseph Hooker and the late Mr. Bentham, upon the great work the "Genera Plantarum," now so happily completed, have been materially lightened by the skilful aid of Professor Oliver, there will, I am sure, be a general feeling of satisfaction that this year his name should also be added to the list of the recipients of the Royal Medals, on which the distinguished names of the more immediate authors of the work are already enrolled.

The Rumford Medal has been awarded to Professor Tobias Robert Thalén, of Upsala, for his spectroscopic researches; his labours in other fields of research, such as elasticity, magnetism, and meteorology, lying outside the limits contemplated by the founder of this medal. Partly in conjunction with Ångström and partly by himself Professor Thalén has produced accurate and elaborate maps, drawn according to the natural scale of wave-lengths, of the spectra of a great number of metals and metalloids. He has also made a careful determination of the absorption-bands of iodine vapour, and of late has been engaged on the difficult problem of determining and properly assigning the spectral lines of bodies of the yttrium and cerium groups, the number of which has recently been so largely augmented by discoveries of new members of those groups, which as yet are only imperfectly studied.

The Davy Medal has been awarded to Professor Hermann Kolbe of Leipzig for his researches in the isomerism of alcohols; but sad to say, though he was aware of the award, he has not lived to receive the medal. While still occupied in his usual avocations, he died suddenly on descending from his carriage at his own door less than a week ago. During a period of upwards of forty years Professor Kolbe devoted his principal attention to some of the most difficult and complicated questions of organic chemistry, many of the important reactions in which have been discovered through his researches. One remarkable result of his study has been that he was able to predict the existence of the chief groups of isomeric alcohols, and even to describe beforehand their characteristic reactions. In one case, at least, his prophecy may, in a certain sense, be said to have fulfilled itself, for it has been by his own experimental evidence that its truth has been confirmed. It may be some slight consolation to his family to think on seeing this medal how highly Professor Kolbe's labours were appreciated, even beyond the limits of his own country.

The Statutes relating to the election of Council and Officers were then read, and Dr. George Harley and Mr. R. H. Inglis Palgrave having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Professor Thomas Henry Huxley, LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Michael Foster, M.A., M.D.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.,

Other Members of the Council.

Captain W. de W. Abney, R.E.; William Henry M. Christie Astron. Royal; Professor George H. Darwin, M.A., F.R.A.S.; Warren De La Rue, M.A., D.C.L.; Robert Etheridge, F.R.S.E., F.G.S.; Sir Frederick J. O. Evans, K.C.B.; Professor William Henry Flower, LL.D.; Professor George Carey Foster, B.A.; Sir Joseph D. Hooker, K.C.S.I.; Professor Henry N. Moseley, M.A., F.L.S.; Hugo Müller, Ph.D.; Captain Andrew Noble, R.A., C.B.; the Lord Rayleigh, D.C.L.; Professor J. S. Burdon Sanderson, LL.D.; Lieutenant-General R. Strachey, R.E., C.S.I.; Professor J. J. Sylvester, M.A., D.C.L., LL.D.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Com-pounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1883 ..	5	44	209	200	64	522
Since Elected ..		+ 5	+ 2		+ 14	21
Since Withdrawn				− 1		− 1
Since Deceased ..		− 3	− 9	− 8	− 1	− 21
Defaulters ..				− 2		− 2
Dec. 1, 1884 ..	5	46	202	189	77	519

Statement of Receipts and Expenditure from November 27, 1883, to November 15, 1884.

	£	s.	d.	£	s.	d.
Balance on hand				7	4	3
Annual Contributions, 197 at £4	£788	0	0	1,022	0	0
" 78 at £3	234	0	0			
Admission Fees and Compositions				130	0	0
Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				223	0	0
Rents:						
Fee Farm Rent, Lewes	18	16	1			
Mablethorpe Estate	104	5	3	123	1	4
Ground Rents				491	15	11
Dividends (exclusive of Trust Funds)				1,616	17	8
" on Jodrell Fund				152	4	10
Interest on Mortgage Loan				587	10	0
Sale of Transactions and Proceedings				815	15	10
Eclipse Expedition				356	10	7
Petty Cash (amount repaid)				0	3	6
				£5,526	3	11

	£	s.	d.	£	s.	d.
Bank Balance				384	10	11
Salaries and Wages				1,097	0	0
Catalogue of Scientific Papers				139	0	0
Books for the Library				132	8	11
Printing Transactions, Parts 2, 1883 to Part 1, 1884, and Separate Copies to Authors and Publisher	424	17	2			
Ditto Proceedings, Nos. 222–232	434	3	1			
Ditto Miscellaneous	77	0	0	1,991	11	5
Paper for Transactions and Proceedings	267	8	9			
Binding ditto	128	13	2			
Advertising ditto	4	9	0			
Engraving and Lithography	655	0	3			
Soirée and Reception Expenses				64	10	10
Coal, Lighting, &c.	78	8	10			
Office Expenses	39	19	7			
House Expenses	290	8	0			
Tea Expenses	20	14	1			
Fire Insurance	41	15	0	612	16	1
Taxes	33	5	10			
Advertising	18	10	6			
Postage, Parcels, and Petty Charges	26	6	4			
Miscellaneous Expenses	63	7	11			
Law Charges (Ground Rents, &c.)				333	13	3
Advanced to Eclipse Expedition				55	11	1
				£4,811	2	6

	£	s.	d.	£	s.	d.
Brought over				5,526	3	11

Trust Funds.

	£	s.	d.	£	s.	d.
Donation Fund Dividends	418	10	6			
Rumford Fund Dividends	68	4	10			
Wintringham Fund „	35	5	0	621	10	3
Copley Medal Fund „	61	6	2			
Davy Medal Fund „	32	6	3			
Croonian Lecture Fund Dividend	5	17	6			
				£6,147	14	2

	£	s.	d.	£	s.	d.
Brought over				4,811	2	6
Donation Fund	463	0	0			
Davy Medal Fund	62	12	0			
Wintringham Fund	35	0	6	622	5	7
Copley Medal Fund	54	14	4			
Bakerian Lecture	4	0	0			
Croonian Lecture	2	18	9			
Balances on hand, Catalogue Account				15	4	3
„ „ Petty Cash				7	15	6
Balance				691	6	4
				£6,147	14	2

JOHN EVANS,
Treasurer.

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £110 per annum.

Ground Rent of House No. 57, Basinghall Street, rent £380 per annum.

" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £500 16*s*. 10*d*. in 1882–83).

£21,000 { £14,952 12*s*. 3*d*. Reduced 3 per Cent. Annuities.
£6,047 7*s*. 9*d*. " " " Handley Fund.

£15,000 Mortgage Loan, 4 per Cent.

£19,314 0*s*. 2*d*. Consolidated Bank Annuities, being £15,861 19*s*. 1*d*., namely :—

	£	*s*.	*d*.
Donation Fund	6,339	0	1
Rumford Fund	2,322	19	0
Wintringham Fund	1,200	0	0
Gassiot Trust	200	0	0
Sir J. Copley Fund	1,666	13	4
General Purposes	4,133	6	8

and £3,452 1*s*. 1*d*. in Chancery, arising from sale of the Coleman Street Estate.

£403 9*s*. 8*d*. New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.

£11,337 17*s*. 3*d*. New Threes { £6,155 2*s*. 5*d*. Scientific Relief Fund.
5,182 14*s*. 10*d*. Jodrell Fund.

Estates and Property of the Royal Society, including Trust Funds—continued.

£667 5*s*. 6*d*. India Fours.

Keck Bequest, £600 Midland 4 %.

£660 Madras Guaranteed 5 per Cent. Railway Stock.—Davy Medal Fund.

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£1,396 Great Northern Railway 4 per Cent. Debentures—The Trevelyan Bequest.

£100 Metropolitan 3½ per Cent. Stock.—Scientific Relief Fund.

£2,200 " " —Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Debentures.—Fee Reduction Fund.

Two Hundred Shares in the Whitworth LandCompany, Limited.—Fee Reduction Fund.

£5,000 Madras Railway Guaranteed 5 % Stock.

£5,000 North Eastern Railway 4 % Stock.

£5,000 London and North Western Consolidated 4 % Preference Stock.

£1,000 Great Northern 4 % Debenture Stock.—Scientific Relief Fund.

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct; and we find that the Balance at the Bankers is £691 6*s*. 4*d*.

FRANCIS GALTON.
HUGO MÜLLER.
G. G. STOKES.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct; and we find that the Balance at the Bankers is £691 6*s*. 4*d*.

JOHN BALL.
J. T. BOILEAU.
JAMES COCKLE.
JOHN RAE.
G. J. SYMONS.

Trust Funds. 1884.

Scientific Relief Fund.

	£	s.	d.
New 3 per Cent. Annuities	6,155	2	5
Great Northern 4 per cent. Debenture Stock	1,000	0	0
Metropolitan 3½ Consols	100	0	0
	£7,255	2	5

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Balance	252	14	3	By Grants	450	0	0
„ Dividends	207	3	1	„ Bought £1,000 Great Northern Debenture Stock	1,223	0	6
„ Siemens' Bequest	1,000	0	0	„ „ £48 14*s.* 8*d.* New 3 °/ₒ	49	3	3
„ Executors of Sir E. Sabine, Medals	49	3	3	„ Balance	9	5	10
„ Sale of £222 3*s.* 5*d.* New 3 °/ₒ	222	9	0				
	£1,731	9	7		£1,731	9	7

Donation Fund.

£6,339 0*s.* 1*d.* Consols.
The Trevelyan Bequest.
£1,396 Great Northern Railway 4 per Cent. Debentures.

	£	s.	d.		£	s.	d.
To Balance	782	19	9	By Grants	463	0	0
„ Dividends	240	17	8	„ Balance	738	10	3
„ Transferred from Handley Fund	177	12	10				
	£1,201	10	3		£1,201	10	3

Rumford Fund.

£2,322 19*s.* Consols.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	67	16	2	By Balance	136	1	0
„ Dividends, 1884	68	4	10				
	£136	1	0		£136	1	0

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s.* 4*d.* Consols.
£403 9*s.* 8*d.* New $2\frac{1}{2}$ per Cent.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	102	8	3	By Gold Medal	4	14	4
„ Dividends, five quarters	12	7	0	„ Sir W. Thomson, Sir J. Copley's Gift	50	0	0
„ Dividend—Sir J. Copley's Fund	48	19	2	„ Bakerian Lecture	4	0	0
				„ Balance	105	0	1
	£163	14	5		£163	14	5

The Keck Bequest.

£600 Midland Railway 4 per Cent. Debenture Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1884	23	10	0	By Payment to Foreign Secretary	23	10	0

Wintringham Fund.

£1,200 Consols.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance, 1883	35	0	6	By Payment to Foundling Hospital, 1884	35	0	6
„ Dividends, 1884	35	5	0	„ Balance	35	5	0
	£70	5	6		£70	5	6

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To Balance, 1883	2	18	9	By Croonian Lecture-fee—Poor of St. James' Parish	2	18	9
„ One-fifth of Rent of Estate at Lambeth Hill, receivable from the College of Physicians	2	18	9	„ Balance	2	18	9
	£5	17	6		£5	17	6

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	108	10	6	By Gold Medals	62	12	0
„ Dividends	32	6	3	„ Balance	78	4	9
	£140	16	9		£140	16	9

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.
£200 3 per Cent. Consols.

	£	s.	d.		£	s.	d.
To Balance	168	15	0	By Payments to Kew Committee	494	4	10
„ Dividends	500	2	4	„ Balance	291	19	8
„ Drawn Bonds	117	7	2				
	£786	4	6		£786	4	6

Handley Fund.

£6,047 7*s*. 9*d*. Reduced.

	£	s.	d.		£	s.	d.
Dividends, 1884	177	12	10	By transferred to Donation Fund	177	12	10

The Jodrell Fund.

£5,182 14*s*. 10*d*. New 3 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Dividends, 1884	152	4	10	By transferred to Royal Society General Account	152	4	10

Fee Reduction Fund.

£2,200 Metropolitan Consols $3\frac{1}{2}$ per Cent.

£7,000 London and North Western Railway 4 per Cent. Debentures.

Two Hundred Shares in the Whitworth Land Company, Limited.

	£	s.	d.		£	s.	d.
To Balance (1883)	347	8	1	By purchase of £400 Metrop. $3\frac{1}{2}$ per Cent.	437	0	0
„ Dividends (1884)	542	10	11	„ transferred to Royal Society General Account (1884)	223	0	0
				„ Balance	229	19	0
	£889	19	0		£889	19	0

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XXXVI, p. 83).

1883–84.

	£
Council of the Royal Society, towards defraying the expenses of the "Eclipse" Expedition	500
Prof. Ramsay and Dr. Young, for apparatus to be used in determining the truth of Prof. James Thomson's theory that the vapour-pressure of a substance in the liquid state is higher than that of the same substance when solid	15
A Committee of the Royal Society, for the purpose of photographing the Corona without an Eclipse, and for carrying out other physical observations at some place of high elevation and of easy access............................	250
W. Lloyd Fox, to provide extra accommodation and instruments for Magnetic Observations in a new Observatory about to be built by the Royal Polytechnic Society of Cornwall (granted contingently on the Society being able to afford to the Council of the Royal Society reasonable assurance of the continuance of the observations)	300
A. W. Hare, for materials and apparatus to be used in an Investigation into the nature and causation of Septicæmia, Gangrene, and Erysipelas	50
Prof. Lankester, for payment of a skilled assistant to aid in carrying on an investigation of the Comparative Histology of the Arthropoda and Mollusca	200
R. Milne Murray, for an investigation into the Physiology of the Pregnant Uterus, with a view to determine (1) the nature of the uterine contraction, (2) the innervation of the uterus, (3) the effect of drugs on the pregnant uterus, (4) the effect of uterine contraction on blood pressure, &c. (materials, assistance, and apparatus not of permanent value)	50
Dr. G. S. Woodhead, for materials and apparatus to aid in an inquiry into the relations of micro-organisms to specific infective diseases, with special reference to the modifying influences which may be brought to act upon the mode and rate of development of these organisms both within and without the body	50
Carried forward...............	£1,415

Brought forward...............	£1,415
D'Arcy W. Thompson, for aid in preparing a Zoological Bibliography (to be printed at the cost of the University of Cambridge)..	50
A. Sedgwick, for aid in publishing a complete illustrated monograph of all known species of Peripatus	50
H. H. Johnston, to supplement a grant made by the British Association, for the exploration of Kilimandjáro and the adjoining Mountains of Tropical Africa	500
A Committee of the Royal Society, for payment of an experienced Botanist to draw up a report on our present knowledge of the Flora of China, and for expense in printing the same	200
F. R. Japp, for an Investigation of the Reactions of Quinones, Diketones, and allied Compounds	75
T. Rupert Jones, for further determination and publication of the fossil Entomostraca	100
V. H. Veley, for an Investigation whether the combinations of the most simple and best defined Molecules occur in simple or more complex proportions	50
A. Gray, T. Gray, and J. J. Dobbie, for continuation of experiments on the relation between the Electrical Conductivity, Specific Inductive Capacity, and Chemical Composition of Glass and allied substances	100
E. Douglas Archibald, for continuation of experimental researches into the Physics of the Atmosphere and its Meteorology, by means of kites	50
Prof. Ramsay and Dr. Young, for apparatus and materials to be used in a Research already in progress on the Relations between Evaporation and Dissociation	50
Prof. G. H. Darwin, towards the cost of publication of a set of computation forms for the reduction of tidal observations by harmonic analysis (granted on the understanding that copies be given gratis to Government Institutions)..........	50
J. T. Bottomley, for expense of a Research on Cooling of Heated Bodies under different circumstances in air and other gases, and in vacuo	75
Dr. Wallich, for experiments connected with the completion of an entirely new form of Condenser for the Microscope	20
A. Buchan, G. Chrystal, and A. Crum Brown (on behalf of the Directors of the Ben Nevis Observatory), for the construction of Self-registering Apparatus for recording the Direction and Force of the Wind	50
Carried forward...........	£2,835

Brought forward................	£2,835
Dr. Schuster, towards the expense of a Research on the Discharge of Electricity through Gases	100
Dr. Tilden, for materials, and payment of an assistant, in Researches on the Phenomena of Solution	100
F. H. Nalder and C. F. Cross, for Researches on the properties of Spiral Springs and Special Applications of the same for the purpose of measurement...............................	25
W. E. Hoyle, for an Investigation into the development of the Cephalopoda, with reference more particularly to the Renal Organ and its connexion with the vascular system	50
Prof. Schäfer, for assistance and apparatus required in Researches into the Physiology of the Nervous System	50
G. R. Vine, for a Research on the Jurassic Polyzoa, their structure, affinities, and distribution	25
A. G. Bourne, for continuation of his Research into the Development of *Myxine glutinosa*	30
H. T. Stainton, in aid of the Publication Fund of the Zoological Record Association	150
Dr. F. Warner, for continuation of a Research on Muscular Movements in the Human Body by a graphic method........	50
J. T. Cunningham, for a Series of Researches on the embryology of Marine Teleostean fishes.	100
A Committee of the Royal Society, in aid of an Expedition for the Exploration of the Mountain of Roraima, in British Guiana..	200
W. Bateson, for aid in Investigating the Anatomy and Development of Balanoglossus in the United States............	100
W. Topley, towards the cost of the preparation and publication of a Geological Map of Europe, under the authority of the International Geological Congress (Third Grant)........	75
T. S. Humpidge, for completion of Researches on Metallic Glucinum	20
C. F. Cross and C. S. Webster, for aid in continuing Researches on the Action of the Halogens upon the Trihydric Phenols	25
W. R. Hodgkinson, for materials and assistance in a Research on Fluorene and its Derivative Difluorenes	25
H. B. Dixon, for Researches (1) on the Velocity of Explosion of Gaseous Mixtures; (2) on a Case of Incomplete Combustion; (3) on the Decomposition of Dry CO_2 by the Electric Spark	200
Carried forward................	£4,160

Brought forward................ £4,160

A Committee appointed in respect of applications made by H. R. Mill and J. Rattray, for a Research upon the General Physical Conditions of the Water, and for aid in Investigating the Algoid Flora of the Firth of Forth.................... 200

H. Tomlinson, for maintenance while continuing his researches on the Influence of Stress and Strain on the Action of Physical Forces.................................... 50

W. E. Adeney, for the determination of the wave-lengths of the lines of the Ultra-violet Spark Spectra of nickel, cobalt, palladium, gold, and platinum.............................. 50

Prof. W. K. Parker, for continuation of Researches into the Morphology of the Vertebrata........................... 300

£4,760

Dr.	£	*s.*	*d.*	*Cr.*	£	*s.*	*d.*
To Balance on hand, Nov. 30, 1883	232	18	9	By Appropriations, as above............	4,760	0	0
To Grant from Treasury	4,000	0	0	Printing, Postage, Advertising, and other Administrative Expenses	64	2	2
To Repayments	232	2	0				
To Interest on Deposit.........	20	1	11				
To Balance	338	19	6				
	£4,824	2	2		£4,824	2	2

Account of Grants from the Donation Fund in 1883–84.

	£
Prof. T. Jeffrey Parker, for the obtaining of Specimens of Hatteria, Apteryx, and other species	75
G. Murray, to aid in his Investigation of the causes of the Salmon Disease	10
Prof. Schäfer, towards the Cost of Researches into the Mechanism of Secretion, and the Physiology of the Heart in Fishes	20
Profs. Reinold and Rücker, for New Apparatus for Investigating the Properties of thin Liquid Films	20
Prof. W. N. Hartley, for construction of an Instrument for the Micrometic Measurement of Spectra	7
Rev. S. J. Perry, for comparison of Kew and Stonyhurst Magnetograms	10
Prof. T. Rupert Jones, for continuing the Illustration of Fossil Entomostraca	25
Messrs. Langley and Gaskell, for assistance in Physiological Investigations on Sauropsida	50
Dr. Brunton, for a Research to be made by Dr. Th. Cash ..	10
J. Murray, for Expenses related to the "Triton" Surveying Expedition	126
The Krakatoa Committee, for Expenses in collecting and classifying notices of the Volcanic Outbursts in the Straits of Sunda	100
	£463

Report of the Kew Committee for the Year ending October 31, 1884.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. Warren De La Rue, *Chairman*.

Captain W. de W. Abney, R.E.
Prof. W. G. Adams.
Capt. Sir F. Evans, K.C.B.
Prof. G. C. Foster.
Mr. F. Galton.
Vice-Adm. Sir G. H. Richards, C.B.
The Earl of Rosse.
Mr. R. H. Scott.
Lieut.-General W. J. Smythe.
Lieut.-Gen. R. Strachey, C.S.I.
Mr. E. Walker.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches.
7th. Miscellaneous.

I. Magnetic Observations.

The Magnetographs have worked uninterruptedly throughout the year.

The curves have been quite free from any large fluctuations, and indeed no unusual disturbance has been registered for a long time past. The most notable perturbations recorded took place on July 3, September 18, and October 2, but the extreme oscillation of the Declination Magnet on any of these days did not exceed 30′, while the change of Horizontal Force was less than 0·02 Gaussian unit.

The values of the ordinates of the different photographic curves determined in January were as follows :—

Declination : 1 inch$=0°\ 22'.04$. 1 mm.$=0°\ 0'.87$.

Bifilar, January 4, 1884,	for 1 inch	$\delta H=0.0276$	foot grain unit.
	„ 1 mm.	„ $=0.0005$	mm. mgr. unit.
Balance, January 4, 1884	„ 1 inch	$\delta V=0.0251$	foot grain unit.
	„ 1 mm.	„ $=0.0005$	mm. mgr. unit.

The distance between the dots of light upon the cylinders of both the Bifilar and Vertical Force Magnetometers having become too small for satisfactory registration, it was found necessary to readjust each instrument, after which the scale values were again determined on January 11th, with the following results :—

The monthly observations with the absolute instruments have been made as usual, and the results are given in the tables forming Appendix I of this Report.

Bifilar	for 1 inch	$\delta H=0\ 0267$	foot grain unit.
	„ 1 mm.	„ $=0\ 0005$	mm. mgr. unit.
Balance	„ 1 inch	$\delta V=0.0296$	foot grain unit.
	„ 1 mm.	„ $=0\ 0005$	mm. mgr. unit.

The tabulation referred to in last year's Report of the traces of the three magnetic elements for the International Polar Commission, is now completed, and the conversion of the results into absolute units is in an advanced stage.

The difficulty experienced in adapting ordinary unprotected gas burners to the Bifilar and Balance Magnetometers, owing to the extremely sensitive nature of the gelatino-bromide paper, has now been overcome by the use of a small screen of blue glass interposed between the flame and mirror. This diminishes the intensity of the light, so that the traces are now well defined instead of being blurred.

Information on matters relating to terrestrial magnetism, and various data, have been supplied to Dr. Wild, General Tillo, Dr. Frolich, Admiral Sir G. Richards, and Dr. Balfour Stewart.

The various magnetic instruments returned by Captain Dawson, R.A., on his arrival in this country from the Fort Rae Circumpolar Expedition, were lent, with the exception of the Balance Magnetometer, one Bifilar, and one Declinometer, to Lieutenant A. Gordon, R.N., of the Meteorological Office, Canada.

Professors Rücker and Thorpe visited the Observatory on July 17, 18, and 19, for the purpose of taking a series of absolute magnetic observations preparatory to surveying the western coast of Scotland, as a preliminary operation towards the proposed repetition of the Survey of Great Britain and Ireland mentioned in last Report.

The following is a summary of the number of magnetic observations made during the year:—

Determinations of	Horizontal Intensity	35
,,	Dip	164
,,	Absolute Declination	63

Soon after the needles of Dip Circle Barrow 33 had been repolished, as mentioned in last Report, it was found that No. 1 worked somewhat indifferently, and on examining its axle slight marks of scoring were discovered. It was therefore deemed advisable to have a new axle substituted. This was accordingly done by Mr. Dover, in December last, and since then the performance of the needle has been satisfactory.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of atmospheric pressure, temperature, and humidity, wind (direction and velocity), bright sunshine, and rain, have been maintained in regular operation throughout the year.

The standard eye observations for the control of the automatic records have been duly registered during the year, together with the daily observations at 0 h. 8 m. P.M. in connexion with the U.S. Signal Service synchronous system. A summary of these observations is given in Appendix II.

The tabulation of the meteorological traces has been regularly carried on, and copies of these, as well as of the eye observations, with notes of weather, cloud, and sunshine have been transmitted weekly to the Meteorological Office.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of	standard barometer	1726
,,	dry and wet thermometers	3452
,,	maximum and minimum thermometers	732
,,	radiation thermometers	1923
,,	rain gauges	732
Cloud and weather observations		1882
Measurements of	barograph curves	8784
,,	dry bulb thermograph curves	9516
,,	wet bulb thermograph curves	8784
,,	wind (direction and velocity)	17568
,,	rainfall curves	576
,,	sunshine traces	2073

In compliance with a request made by the Meteorological Council

to the Committee, the Meteorological instruments at the Observatories of Armagh, Falmouth, Oxford (Radcliffe), and Valencia have been inspected by Mr. Whipple, and those at Aberdeen and Stonyhurst by Mr. Baker, during their respective vacations.

Assistance has also been given in arranging the plans, designs, &c., for the New Observatory, now in progress of erection at Falmouth.

With the concurrence of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to, and published by "The Times" and "The Torquay Directory." Data have also been supplied to the Council of the Royal Meteorological Society, the editor of "Symons's Monthly Meteorological Magazine," the Secretary of the Institute of Mining Engineers, Messrs. Gwilliam, Mawley, Rowland, and others. The cost of these abstracts is borne by the recipients.

The weekly abstracts of meteorological results, which have been published by the "Illustrated London News" without interruption since 1856, were discontinued in July last, at the request of the proprietors, owing to changes being introduced in the form of publication of the paper.

Electrograph.—This instrument was temporarily dismounted in May, whilst some repairs and painting of the instrument room were in progress, and recently some trouble has been experienced in keeping the potential of the charge constant, otherwise it has been maintained in continuous action.

The tabulation of the curves is at present in arrear, not having been completed beyond February 28, 1882.

Its scale value has been redetermined on two occasions by means of the Portable Electrometer, White No. 53, such determinations being necessary after every readjustment of the instrument.

III. Solar Observations.

The sketches of Sun-spots, as seen projected on the photoheliograph screen, have been made on 185 days, in order to continue Schwabe's enumeration, the results being given in Appendix II, Table IV.

A few experiments were made in June with the Photoheliograph, with a view of testing the suitability of certain plates prepared by Messrs. Morgan and Kidd for solar photography. With this exception nothing has been done in that branch during the year.

Transit Observations.—Frequent observations of both solar and sidereal transits have been made, for the purpose of keeping correct local time at the Observatory.

Numerous clock and chronometer comparisons have been also made. The Observatory Chronometers, Parkinson and Frodsham No. 2408, and Molyneux No. 2125, have been cleaned and readjusted,

and the following clocks are kept carefully rated in addition as time-keepers of the Observatory:—French; Shelton K. O.; Shelton No. 35; and Dent 2011. By the courtesy of Mr. Preece, Superintendent of Telegraphs, the Richmond Chief Post Office was placed in direct communication with the Royal Observatory, Greenwich, on January 22, and enabled to receive the time signal at 10 A.M., when a period of cloudy weather had rendered the true time a little uncertain. Two chronometers conveyed to the Post Office, showed, on comparison with the signal, a satisfactory agreement between the times as kept at the two Observatories.

IV. Experimental Work.

Actinometry.—A report on the Balfour Stewart actinometer observations made last year, was submitted in December to the Meteorological Council, at whose expense the observations were carried on, and it was resolved to discontinue them. The instrument has since been returned to Professor Balfour Stewart at his request.

Fog Gauge.—In conformity with a suggestion contained in an article in "Symons's Meteorological Magazine," vol. xviii, p. 58, a painted board has been set up to the north of the Observatory, to serve as a gauge for measuring the intensity of fogs.

Since its erection in January last no fog, however, has been observed of intensity 1 on its scale.

Magnetic Survey of Great Britain and Ireland.—With reference to this, the Sub-Committee appointed last year has now under consideration the details necessary for the early prosecution of the survey.

Professors Rücker and Thorpe have during the past summer made preliminary observations at a number of stations along the West Coast of Scotland, their base observations being made at the Observatory as above stated.

Nocturnal Radiation.—Experiments have been made with a new pattern thermometer, designed by Messrs. Negretti and Zambra for observations of nocturnal terrestrial radiation, with a view to the avoidance of several serious defects in the Rutherford Minimum, now generally used. Very favourable results were obtained until the instrument was damaged, and had to be sent back to the makers. It has not yet been returned to the Observatory.

Photo-nephograph.—Various experiments have been made with this apparatus during the year, but in consequence of the short base line obtainable with the small amount of connecting wire available for working the pair of cameras, very few satisfactory determinations of cloud altitudes have been made.

A report having, however, been submitted to the Meteorological Council, that body has granted a sum of 40*l.* to the Committee

for the purpose of purchasing a half-mile of double wire telegraphic cable and reel, together with switches and keys, in order that the two cameras may be worked at a distance of 800 yards apart.

A stand has been erected on the roof of the Observatory, where camera A will be permanently placed, and camera B will be similarly supported by another permanent stand at the other end of the cable. Both cameras being oriented with reference to the same point of the horizon, the distant observer will be instructed as to the direction and elevation of his instrument by means of a telephone switched on to the line for the purpose.

Some difficulties having been met with in working the electrical instantaneous shutters, part of the apparatus was returned to the makers, the Philosophical Instrument Construction Society, Cambridge, and rectified.

Experiments with the new arrangement are now being made, and should they prove successful it is intended to bury the cable in the ground across the park beside the Observatory gas main, thereby obviating the present necessity of laying out and winding it in again every time it is desirable to make cloud altitude and air-current motion determinations.

Solar Radiation Thermometers.—The experiments with a view to determining the causes of variation in the readings of similarly constructed and exposed black bulb thermometers, *in vacuo*, have been continued during the year.

The first series of observations having been concluded, and the results communicated to the Royal Meteorological Society and published in their "Quarterly Journal," vol. x, p. 45, the six thermometers were returned to Messrs. Negretti and Zambra, in order that all might have their bulbs coated with three coats of lampblack and their jackets altered: one pair is now enclosed in small bulbs, a second pair in medium, and the third pair in large bulbs.

With the exception of one which was accidentally broken in July, they have been read daily since May 3. The results have not yet been fully discussed, but a cursory inspection appears to indicate that the larger the containing bulb the lower is the reading of the enclosed blackened bulb thermometer.

Ventilation Experiments.—Assistance has been given to a Sub-Committee of the Sanitary Institute in their experiments on the motion of air in ventilating tubes, which have been carried on during the summer under the charge of Mr. R. Rymer Jones, C.E., in a hut erected for the purpose, adjacent to the Observatory.

The experiments are in continuation of those prosecuted in the Experimental House in 1880.

Wind Integrator.—At the request of Mr. Walter Baily, M.A., a wind-component integrator of his invention, described in the "Phil. Mag.,"

vol. xvii, p. 482, has been erected in the Experimental House, being attached by permission of the Meteorological Council to their spare Beckley Anemograph.

Some difficulties were experienced on account of the unsuitability of the electrical counters fitted to it for registration of light winds, but these have now been overcome, and the instrument is working satisfactorily.

V. Verification of Instruments.

The following magnetic instruments have been verified, and their constants determined:—

3 Unifilar Magnetometers for Elliott Brothers, London.
3 Dip Circles for Elliott Brothers, London.
1 Unifilar Magnetometer for Negretti and Zambra, London.
1 Dip Circle for Negretti and Zambra, London.

There have also been purchased on commission and verified:—

- A Unifilar Magnetometer and a Dip Circle for Professor Rücker, Leeds College of Science.
- A small Robinson's Pattern Dip Circle for Senhor Capello, Lisbon.
- 2 Fox Circles with Gimbal Tables complete, for the United States Government.
- 6 small Collimating Magnets for Professor Tacchini, Rome.
- One Pair of Dip Needles for the Greely Relief Expedition.
- A Dip Circle for Dr. Wild, St. Petersburg.
- A set of self-recording Magnetometers for the United States Government.
- A Unifilar and a Pair of Dip Needles are at present undergoing examination.

The General Verification Department continues in full activity, a considerable increase having taken place in the number of Sikes' Hydrometers and Sextants examined.

The total number of instruments tested in the past year was as follows:—

Barometers, Standard	44
,, Marine and Station	80
Aneroids	84
Total	208

Thermometers, ordinary Meteorological	.....	1225
,,	Standard	83
,,	Mountain	164
,,	Clinical	8726
,,	Solar radiation	42
	Total	10240

Hydrometers	1161
Anemometers	2
Rain Gauges	3
Sextants	64
Index and Horizon Glasses, unmounted	87
Dark Glasses, unmounted	254

Besides these, 13 Deep-sea Thermometers have been tested, 4 of which were subjected, in the hydraulic press, without injury, to pressures exceeding two tons on the square inch. 142 Thermometers have been compared at the freezing-point of mercury, making a total of 10395 for the year.

Duplicate copies of corrections have been supplied in 30 cases.

The number of instruments rejected on account of excessive error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers, clinical	40
,, ordinary meteorological	2
Various	27

10 Standard Thermometers have also been calibrated, and supplied to societies and individuals during the year.

A Thermograph has been examined, and had its scale values determined, for the Japanese Government. A Richard Temperature Recorder, a Self-registering Aneroid, an Electrograph, and a Richard Humidity Recorder, have also been tried.

There are at present in the Observatory undergoing verification, 32 Barometers, 742 Thermometers, 14 Hydrometers and 10 Sextants.

VI. Rating of Watches.

The arrangements for rating watches mentioned in last year's Report have been completed and brought into operation successfully, at a cost of £193.

A second safe having been purchased by the Committee, an apparatus was fitted to it which enables the enclosed watches to be maintained continuously at either high temperatures, without being subjected to injury by fumes of gas, or at low temperatures.

Two additional Mean Time Clocks have been obtained, one of them, a Transit of Venus Expedition Clock, Dent 2011, has been lent to the Committee by the Astronomer Royal; the other has been purchased. Mr. T. Mercer, watch manufacturer, of Coventry, having obligingly placed a number of watches at the disposal of the Superintendent, two dozen were obtained on loan from him, and were daily compared, tested, and rated by the assistants for three months. This enabled them to become familiarized with the work of rating before watches were received from the public.

The Superintendent, after communicating with the Directors of the Geneva and the Yale Observatories, prepared a circular specifying the conditions watches must fulfil in order to obtain certificates of the various classes, A, B, and C, which are issued, and the nature of the test to which they will be subjected. This circular, together with the forms of certificates, &c., after revision and approval by the Committee, was printed, and copies forwarded to all the leading watch manufacturers of this country, as well as to the principal journals, many of which very favourably noticed the scheme.

Rating commenced on May 13, and up to the present 42 watches have been tried, of which 22 were submitted by the owners, and 20 by the manufacturers, or by dealers.

Certificates have been awarded to 17 of these watches, 7 are now on trial.

The following table will indicate the nature of the trials to which the certificates refer :—

Position of watch during test.	For certificate of Class		
	A.	B.	C.
Vertical, with pendant up	10 days	14 days	8 days
„ „ „ right	5 „	—	—
„ „ „ left	5 „	—	—
Horizontal, with dial up	5 „	14 days	8 days
„ „ „ down	5 „	—	—
„ at temp. 85° F.	5 „	1 day	—
„ „ 35° F.	5 „	1 „	—
Not rated	5 „	1 „	—
Total duration of test	45 days	31 days	16 days

VII. Miscellaneous.

Photographic Paper, &c.—This has been supplied to the Observatories at Batavia, Coimbra, Colaba, Mauritius, Stonyhurst, and St. Petersburg, and to the Meteorological Office.

Blank Magnetic Observation Forms have been supplied to Professors Brioschi and Rücker, also to Messrs. Negretti and Zambra.

A glass scale for measuring anemograph curves was constructed for the Royal Alfred Observatory, Mauritius.

Two glass scales graduated in millimeters for the purpose of tabulating magnetic curves were constructed for the Toronto Observatory, and also twelve paper scales were supplied for the magnetometers.

A level with spare bubbles has been supplied to Dr. E. van Rijckevorsel, and a hemi-cylindrical lens to Dr. Wild, St. Petersburg.

Exhibition.—A number of instruments of interest were exhibited at the Fifth Annual Exhibition of the Royal Meteorological Society, which was devoted to thermometers and thermometry, and held in the rooms of the Institution of Civil Engineers in March last.

International Health Exhibition.—The Committee exhibited specimens of the certificates issued with instruments verified at the Observatory, as well as diagrams showing the number of thermometers tested annually since 1870, and also indicating the improvement in quality of such instruments.

Workshop.—The several pieces of Mechanical Apparatus, such as the Whitworth Lathe and Planing Machine, procured by Grants from either the Government Grant Funds or the Donation Fund for the use of the Kew Observatory, have been kept in thorough order.

Library.—During the year the Library has received, as presents, the publications of—

31 English Scientific Societies and Institutions, and

76 Foreign and Colonial Scientific Societies and Institutions.

Several volumes of duplicates of works on Astronomy, Terrestrial Magnetism, and Meteorology, have been presented to the Electrical Library of the Franklin Institute, Philadelphia. Others have also been disposed of to various individuals.

Additional shelves have been provided to afford more room, which was urgently required.

House, Grounds, and Footpath.—These have all been kept in order during the year. The iron fencing round the building has been painted, a wall on each side of the entrance steps has been erected, and the necessary external repairs have been effected by Her Majesty's Commissioners of Works.

Personal Establishment.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant and Magnetic Observer.
J. Foster, Verification Department.

H. McLaughlin, Librarian and Accountant.
E. G. Constable, Solar Observations and Watch Rating.
T. Gunter, Verification Department.
W. Boxall, Photography and Tabulation.
E. Dagwell, Watch Rating ,, ,,
C. Henley, H. A. Widdowson, H. Barton — Verification Department.
M. Baker, Messenger and Care-taker.

E. Coates resigned his duties in September.

C. Bell was temporarily employed in December as additional assistant in Verification Department.

Abstract. *Kew Observatory Receipts and Payments Account from November 1, 1883, to November 1, 1884.*

Dr. RECEIPTS.

		£ s. d.
To Balance from 1882–83		547 4 11
Royal Society (Gassiot Trust)		494 4 10
Meteorological Office		400 0 0
Meteorological Office, for Postages, &c.		8 3 4
Verification Fees, Meteorological Office	£86 19 6	
,, ,, Observatories and Institutions	57 18 6	
,, ,, Instrument Makers and others	614 6 11	
		759 4 11
Instruments on Commission		686 5 5
Sale of Waxed Paper		33 8 0
,, Standard Thermometers	18 19 6	
,, Forms, &c.	9 1 1	
Copying Registers	56 0 0	
		84 0 7
Experimental Work for Meteorological Office		19 0 7
International Circumpolar Committee		25 0 0
Watch Rating Fees, &c.		18 13 6
		£3075 6 1

PAYMENTS. *Cr.*

		£ s. d.
By Salaries	£1071 4 6	
Extra Payments	155 0 9	
		1226 5 3
Fuel and Gas	65 0 2	
Furniture and Fittings	11 19 4	
Chandlery, &c.	9 5 2	
Painting and Repairs	108 2 2	
Rent and Incidentals (Enclosure and Path)	24 2 1	
		218 8 11
Printing and Stationery (General)	25 2 9	
,, ,, (Verification Department)	14 15 0	
Postages	18 11 10	
Library	13 15 8	
Messenger and Housekeeper	57 4 0	
Porterage and Contingencies	22 7 5	
		151 16 8
Purchase of Chemicals and Materials	37 17 6	
,, ,, Tubes for Standard Thermometers	3 3 0	
,, ,, Anemograph Sheets	1 1 0	
Verification Department Expenses (Ice, Carbonic-Acid Gas, &c.)	20 10 9	
Repair of Instruments, and Purchase of New	14 12 4	
Carpenter's Work and Sundries	13 7 9	
		90 12 4
Postages and Payments on behalf of Meteorological Office		10 4 3
Instruments purchased on Commission		741 18 8
Purchase of Waxed Paper, Packing ditto, &c.		20 0 5
Payments on behalf of Experimental Work		7 10 3
,, ,, International Circumpolar Committee		16 0 3
,, ,, Watch Rating		110 2 1
Balance—Bank of England	411 8 6	
London and County Bank	55 0 0	
Cash in hand	15 18 6	
		482 7 0
		£3075 6 1

November 13, 1884. Examined and compared with the Vouchers, and found correct. (Signed) FRANCIS GALTON, *Auditor.*

ASSETS.

	£ s. d.
By Balance as per Statement	482 7 0
Meteorological Office, Allowances and Sundries	38 1 5
Verification Fees due	80 15 0
Watch Rating Fees due	1 6 3
Photographic Paper	44 15 4
Commissions, &c.	12 12 5
Blank Forms	8 10 0
Standard Thermometers	91 4 0
International Circumpolar Committee	8 19 9
	£768 11 2

LIABILITIES.

	£ s. d.
To Gas, Fuel, and House Account	8 7 8
Apparatus, Chemicals, &c.	3 16 4
Commissions, &c.	11 1 0
Balance	745 6 2
	£768 11 2

November 18, 1884. (Signed) G. M. WHIPPLE.

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N. Long.* $0^h\ 1^m\ 15^s\!\cdot\!1$ *W., for the year October* 1883 *to September* 1884.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnets 101 B and N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are $3\frac{1}{2}$ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0\cdot0001194(t_0-35)+0\cdot000,000,213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is $-0\cdot00129$.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1,250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observations have been made and reduced by Mr. T. W. Baker.

Vibration Observations for Absolute Measure of Horizontal Force.

Table I.

Month.	G. M. T.	Temperature. Fahr.	Time of one Vibration.*	Log mX. Mean.	Value of m.†
1883.	d. h. m.	°	secs.		
October..........	26 12 4 P.M.	60·7	4·6518		
	3 3 P.M.	61·0	4·6500	0·30920	0·52128
November........	27 11 44 A.M.	47·9	4·6480		
	28 12 38 P.M.	60·3	4·6534	0·30880	0·52093
December........	28 11 22 A.M.	44·3	4·6478		
	2 42 P.M.	47·1	4·6478	0·30882	0·52110
1884. January..........	29 11 26 A.M.	49·5	4·6488		
	2 58 P.M.	56·7	4·6477	0·30919	0·52060
February	22 11 8 A.M.	51·8	4·6508		
	3 2 P.M.	55·5	4·6494	0·30887	0·52054
March...........	31 11 47 A.M.	53·0	4·6524		
	3 9 P.M.	58·5	4·6500	0·30877	0·52036
April............	29 11 10 A.M.	58·3	4·6530		
	3 8 P.M.	69·3	4·6522	0·30900	0·52051
May.............	29 11 35 A.M.	53·8	4·6473		
	3 26 P.M.	59·2	4·6487	0·30934	0·52015
June	30 11 19 A.M.	73·0	4·6567		
	3 6 P.M.	78·6	4·6553	0·30910	0·52015
July	30 11 5 A.M.	72·2	4·6537		
	2 57 P.M.	77·0	4·6563	0·30924	0·52019
August	28 11 37 A.M.	71·3	4·6568		
	3 47 P.M.	75·0	4·6533	0·30912	0·52019
October..........	1 11 44 A.M.	60·9	4·6548		
	3 10 P.M.	66·3	4·6520	0·30886	0·51970

* A vibration is a movement of the magnet from a position of maximum displacement on one side of the meridian to a corresponding position on the other side.

† m = magnetic moment of vibrating magnet.

Observations of Deflection for Absolute Measure of Horizontal Force.

Table II.

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$. Mean.
1883.	d. h. m.	foot.	°	° ′ ″	
October........	26 12 46 P.M.	1·0	61·4	15 23 8	9·12494
		1·3	....	6 56 32	
	2 19 „	1·0	61·5	15 22 55	
		1·3	....	6 56 39	
November......	27 12 33 P.M.	1·0	50·0	15 23 7	9·12476
		1·3	....	6 57 18	
	28 12 0 „	1·0	57·7	15 24 1	
		1·3	....	6 56 43	
December......	28 12 10 P.M.	1·0	44·9	15 25 45	9·12502
		1·3	....	6 57 30	
	2 5 „	1·0	46·1	15 25 37	
		1·3	....	6 57 50	
1884.					
January........	29 12 15 P.M.	1·0	52·0	15 22 18	9·12382
		1·3	....	6 56 20	
	2 18 „	1·0	55·6	15 20 52	
		1·3	....	6 55 32	
February	22 12 2 P.M.	1·0	53·8	15 22 39	9·12402
		1·3	....	6 56 5	
	2 20 „	1·0	56·2	15 21 36	
		1·3	....	6 55 46	
March	31 12 28 P.M.	1·0	54·8	15 22 3	9·12382
		1·3	....	6 56 2	
	2 30 „	1·0	59·2	15 20 25	
		1·3	....	6 55 20	
April..........	29 12 1 P.M.	1·0	60·1	15 21 20	9·12386
		1·3	....	6 56 2	
	2 19 „	1·0	66·0	15 19 9	
		1·3	....	6 54 46	
May	29 12 26 P.M.	1·0	55·8	15 19 43	9·12292
		1·3	....	6 54 49	
	2 39 „	1·0	58·3	15 19 10	
		1·3	....	6 54 40	
June	30 12 7 P.M.	1·0	72·7	15 18 26	9·12315
		1·3	....	6 54 10	
	2 19 „	1·0	76·7	15 15 55	
		1·3	....	6 53 27	
July	30 11 55 A.M.	1·0	73·8	15 16 24	9·12308
		1·3	....	6 54 1	
	2 5 P.M.	1·0	77·6	15 16 30	
		1·3	....	6 53 42	
August	28 12 11 P.M.	1·0	71·3	15 17 59	9·12320
		1·3	....	6 54 24	
	2 11 „	1·0	74·3	15 16 52	
		1·3	....	6 53 46	
October........	1 12 30 P.M.	1·0	62·6	15 19 52	9·12264
		1·3	....	6 53 1	
	2 28 „	1·0	65·0	15 18 0	
		1·3	....	6 54 2	

Dip Observations.—Table III.

Month.	G. M. T.	Needle.	Dip.
			North.
1883.	d. h. m.	No.	° ′
Oct.	25 2 37 P.M.	1	67 40·19
	2 38 „	2	39·84
	27 2 34 „	1	39·37
	2 33 „	2	40·60
	Mean..	....	67 40·0
Nov.	26 3 18 P.M.	1	67 40·0
	3 18 „	2	41·19
	27 2 42 „	1	41·09
	2 42 „	2	41·88
	28 3 0 „	1	41·60
	3 0 „	2	41·84
	Mean..	....	67 41·27
Dec.	17 2 40 P.M.	1	67 40·31
	2 38 „	2	40·03
	18 2 31 „	1	40·37
	2 30 „	2	41·50
	19 3 6 „	1	40·25
	3 4 „	2	40·47
	Mean..	....	67 40·49
1884. Jan.	23 3 8 P.M.	1	67 39·47
	3 8 „	2	40·90
	24 2 22 „	1	39·40
	2 21 „	2	40·48
	Mean..	....	67 40·06
Feb.	19 2 58 P.M.	1	67 39·81
	2 59 „	2	39·72
	20 3 3 „	1	39·81
	3 4 „	2	40·28
	Mean..	...	67 39·90
Mar.	26 2 58 P.M.	1	67 38·97
	2 58 „	2	38·22
	27 2 54 „	1	37·72
	2 53 „	2	38·59
	Mean..	....	67 38·37
			North.
1884.	d. h. m.	No.	° ′
April	24 3 4 P.M.	1	67 37·69
	3 6 „	2	39·28
	25 3 8 „	1	39·84
	3 3 „	2	40·18
	Mean..	....	67 39·25
May	24 12 14 P.M.	1	67 38·50
	17 „	2	39·53
	26 2 28 „	1	37·93
	2 28 „	2	38·84
	Mean..	....	67 38·70
June	24 2 57 P.M.	1	67 37·90
	2 56 „	2	38·34
	25 3 0 „	1	36·65
	3 0 „	2	38·50
	Mean..	....	67 37·85
July	28 2 43 P.M.	1	67 39·40
	2 43 „	2	38·81
	31 2 48 „	1	39·31
	2 48 „	2	37·90
	Mean..	....	67 38·85
Aug.	26 2 47 P.M.	1	67 38·81
	2 52 „	2	39·31
	27 2 45 „	1	38·65
	2 44 „	2	39·03
	Mean..	....	67 38·95
Sept.	29 2 42 P.M.	1	67 40·62
	2 42 „	2	67 39·28
	Mean..	....	67 39·95

Table IV.

Month.	Declination.	Magnetic Intensity.								
	Mean of Observations.	English Units.			Metric Units.			C. G. S. Measure.		
		X, or Horizontal Force.	Y, or Vertical Force.	Total Force.*	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
	West.									
1883. October	18° 33′ 57″	3·9096	9·5168	10·2885	1·8026	4·3880	4·7438	0·1803	0·4388	0·4744
November	18 35 18	3·9086	9·5242	10·2951	1·8022	4·3915	4·7469	0·1802	0·4391	0·4747
December	18 34 14	3·9075	9·5155	10·2866	1·8017	4·3874	4·7430	0·1802	0·4387	0·4743
1884. January	18 31 24	3·9145	9·5293	10·3020	1·8049	4·3938	4·7501	0·1805	0·4394	0·4750
February......	18 32 55	3·9122	9·5223	10·2946	1·8038	4·3906	4·7467	0·1804	0·4391	0·4747
March........	18 37 8	3·9127	9·5115	10·2849	1·8041	4·3856	4·7422	0·1804	0·4386	0·4742
April	18 33 23	3·9135	9·5205	10·2918	1·8045	4·3898	4·7454	0·1805	0·4390	0·4745
May..	18 33 18	3·9193	9·5302	10·3046	1·8071	4·3942	4·7513	0·1807	0·4394	0·4751
June	18 33 25	3·9171	9·5183	10·2927	1·8061	4·3887	4·7458	0·1806	0·4389	0·4746
July..........	18 30 52	3·9181	9·5286	10·3027	1·8066	4·3935	4·7504	0·1807	0·4394	0·4750
August	18 33 55	3·9171	9·5266	10·3005	1·8061	4·3926	4·7494	0·1806	0·4393	0·4749
September	18 32 4	3·9184	9·5378	10·3115	1·8067	4·3977	4·7544	0·1807	0·4398	0·4754

APPENDIX II.

Meteorological Observations.—Table I.

Mean Monthly results.

Months.	Thermometer. Mean.	Means of— Max.	Means of— Min.	Means of— Max. and Min.	Absolute Extremes. Max.	Date.	Min.	Date.	Barometer.* Mean.	Absolute Extremes. Max.	Date.	Min.	Date.	Mean vapour-tension.
1883.						d. h.		d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Oct.....	50°·5	56°·4	44°·8	50°·6	62°·6	14 1 P.M.	38°·4	21 0·25 A.M.	29·983	30·544	8 10 A.M.	29·298	17 6 A.M.	·309
Nov.	43·4	49·1	37·5	43·3	56·0	6 11 A.M.	29·3	{13 7 ,, / 15 8 ,,}	29·850	·424	28 10 P.M.	28 975	6 8 ,,	·243
Dec.	40·7	44·3	36·5	40·4	54·0	{3 7 P.M. / 13 {2 ,, / 5 ,,}}	29·4	6 5 P.M.	30·175	·605	25 10 A.M.	29·431	11 3 ,,	·213
1884.														
Jan.....	43·9	47·6	39·8	43·7	54·2	23 4 ,,	32·1	1 4 A.M.	30·106	·674	16 11 ,,	28·544	26 7 P.M.	·245
Feb.	42·2	47·0	37·5	42·3	54·8	13 2 ,,	29·5	3 6 ,,	29·923	·503	3 8 ,,	29·377	1 9 P.M.	·223
March..	43·9	50·7	37·7	44·2	65·3	15 3 ,,	27·2	1 3 ,,	29·945	·280	5 Midt.	·152	10 6 A.M.	·223
April...	44·7	52·0	37·5	44·8	65·4	2 2 ,,	28·1	23 5 ,,	29·831	·113	13 11 P.M.	·281	4 Midt.	·221
May ...	53·5	62·8	44·5	53·7	76·7	24 2 ,,	35·5	1 5 ,,	30·006	·456	22 8 A.M.	·356	3 7 P.M.	·285
June...	58·1	66·7	50·1	58·4	80·0	27 4 ,,	40·9	1 1 ,,	30·041	·342	12 Midt.	·522	7 5 A.M.	·349
July ...	62·8	71·9	54·2	63·1	83·6	4 {1 ,, / 2 ,,}	43·2	26 5 ,,	29·960	·217	1 11 A.M.	·614	10 3 P.M.	·423
Aug....	64·1	75·0	53·9	64·5	89·2	11 3 ,,†	46·6	26 5 ,,	30·014	·277	5 8 ,,	·660	29 5 A.M.	·410
Sept....	59·1	66·8	51·8	59·3	80·0	17 2 ,,	40·6	30 6 ,,	30·016	30·386	18 {9 ,, / 10 ,,}	29·410	4 5 ,,	·401
Means..	50·6	57·5	43·8	50·7	..	....	..	....	29·988	..	....	..	....	·295

The above Table is extracted from the Publications of the Meteorological Office, by permission of the Meteorological Council.

* Reduced to 32° at M.S.L. † Approximate.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *. Total.	Rainfall *. Maximum.	Rainfall *. Date.	Weather. Number of days on which were registered: Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	Wind †. Number of days on which it was: N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Variable.	Calm.
1883.		in.	in.																		
October ..	7·0	1·750	0·420	15	13	..	..	..	..	11	..	5	3	2	1	3	6	5	3	3	10
November	6·0	2·540	0·345	4	19	..	1	..	2	9	..	1	2	..	1	6	6	7	2	5	8
December	8·3	0·660	0·340	10	17	3	..	..	1	19	1	5	2	2	2	..	3	9	5	3	8
1884.																					
January ..	8·2	2·295	0·605	26	15	2	2	..	2	25	4	1	1	1	..	1	12	9	2	4	7
February .	7·1	1·400	0·350	1	13	..	2	1	..	15	1	1	1	6	..	5	8	4	1	3	4
March ...	6·5	1·240	0·460	10	8	..	..	..	3	11	..	2	6	3	..	6	4	4	3	3	5
April	6·7	1·255	0·540	6	10	..	2	2	1	12	..	2	12	3	1	4	2	2	1	3	5
May	5·1	0·635	0·240	5	9	..	3	2	9	10	..	1	4	6	..	1	11	6	1	1	2
June	6·4	2·200	0·890	6	8	..	1	3	4	12	..	6	6	3	..	1	3	2	5	4	7
July	6·7	2·240	0·880	6	16	..	1	2	1	13	..	2	..	..	2	4	13	6	1	3	6
August ..	4·5	0·960	0·300	27	9	..	..	1	9	7	..	3	2	5	1	4	7	3	1	5	9
September	5·4	1·690	0·670	3	15	..	..	..	5	9	..	1	4	5	1	4	8	6	..	1	4
Totals..		18·865			152	5	12	11	37	153	6	30	43	36	9	39	83	63	25	38	75

* Measured daily at 10 A.M. by gauge 1·75 feet above surface of ground. † As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.*				Maximum temperature in sun's rays. (Black bulb *in vacuo.*)			Minimum temperature on the ground.			Horizontal movement of the Air.†			
	Total number of hours recorded.	Percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest.	Date.	Mean.	Lowest.	Date.	Average hourly Velocity.	Greatest hourly Velocity.	Date.	Hour.
1883.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.		
October	86 0	26	9 36	2	91	117	2	40·2	28·0	23	9	34	4	Noon.
November	80 48	30	6 0	10	79	92	9	31·5	19·4	14, 15	9	33	25	10 A.M.
December	31 54	13	4 48	4 & 7	62	84	13	32·6	22·3	5	11	43	12	3 A.M.
1884.														
January	29 24	11	6 36	28	67	82	29	35·0	26·7	13	12	53	26	7 P.M.
February	54 12	19	6 48	18	80	99	20	32·8	21·2	27	13	38	2	Noon.
March	108 0	29	9 6	16	93	113	18	30·9	18·8	3	9	28	31	2 P.M.
April	98 30	24	8 30	9	104	125	8	31·0	16·1	23	9	31	17	5 P.M.
May	208 6	43	13 24	11	117	130	24	39·0	27·7	1	12	30	4	1 P.M.
June	157 0	32	13 24	12	119	130	21	45·2	26·3	1	7	25	2	6 P.M.
July	153 6	30	13 6	2	127	141	8	49·0	35·5	26	8	30	14	4 P.M.
August	227 6	52	12 24	4	126	138	18	47·4	37·3	26	7	22	31	4 P.M.
September ..	129 24	36	11 0	5	111	124	2	46·6	33·5	30	9	27	7	10 A.M.

* Registered by the sunshine-recorder.

† As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days without spots.
1883.			
October	20	19	0
November	21	18	0
December	12	13	0
1884.			
January	9	20	0
February	17	20	0
March	15	15	0
April	16	15	0
May	20	16	0
June	10	9	0
July	13	13	0
August	18	10	0
September	14	10	0
Totals	185	178	0

APPENDIX III.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Old Kew Thermometer Screen	1868
	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Dr. T. Thorpe, F.R.S.	Three Open Scale Standard Thermometers, Nos. 561, 562, and 563.	1879
	Tripod Stand	1883
Major Herschel, R.E., F.R.S.	Invariable Pendulums, Nos. 1821, 4, and 11, Shelton Clock, R.S. No. 34. Stands, and Accessories.	1881
Mr. R. W. Munro ..	Standard Straight-edge....................	1881
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
Major-General Sir H. Lefroy, R.A., F.R.S.	Two parcels Magnetical and Meteorological MSS. from the Sabine Magnetic Office.	1882
Dr. E. van Rijckevorsel	Dip-Circle by Barrow, No. 24, complete, with four Needles, and a Pair of Magnetizing Bars.	1883
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.	1883
Professor O. J. Lodge	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Mr. W. F. Harrison.	Condensing lens and copper lamp chimney ..	1883

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Charles William Siemens, whose untimely death on November 19, 1883, we have to record, was born at Lenthe, in Hanover, on the 4th April, 1823.

His education was begun at the Gymnasium of Lübeck, from which he passed to the Polytechnic School of Magdeburg, and finished at the University of Göttingen. During his University course he had the advantage of studying the natural sciences under Professors Wöhler and C. Himly, from the latter of whom the Siemenses received the first impulse towards those investigations which led to their first invention—the electro-gilding process.

Leaving the University in 1842, at the age of nineteen, William Siemens entered upon the practical business of life by spending a year as pupil in the engine works of Count Stolberg, thus adopting the profession of engineer, in which in after years he attained such commanding eminence. It is interesting to observe that, at the very outset of his career, he showed how wide and all-embracing was his understanding of the word engineer, and of what an engineer should be. To his mind the word meant the practical development of the results of science, in order to minister to the wants and comforts of mankind; and he considered that an engineer must be able, if not himself an original investigator, at least to appreciate fully the conquests of science in the realm of Nature, and from his knowledge of ways and means to adapt the results so gained from time to time to the needs of life. His career shows how completely William Siemens realised this idea, and not only so, but establishes for him strong claims to rank as an able worker in the domain of pure science.

In 1843, when he was only twenty years old, William Siemens came to England to realise a joint invention of his own and his brother Werner in electro-gilding; and, persevering through the complication of difficulties naturally met with by a young man in a strange land, with little knowledge of its language, he succeeded in proving the usefulness of the invention, and getting it carried into practical effect through the wise and kindly appreciation of Mr. Elkington. Encouraged by this success, William Siemens returned a year later with his chronometric governor—an invention of remarkable beauty and ingenuity, in which, by the motion of a pivoted framework carrying an idle wheel geared to bevel wheels on two shafts in line, or geared to the outer and inner circumferences of concentric wheels, rotating in opposite directions on coaxial shafts, the movement of one wheel is caused to keep time with that of the other. We believe that, although the invention was not a commercial success, and is not generally known in this country as practically realised except in its application to regulate the motions of

chronoscopic instruments in the Royal Observatory at Greenwich, it may yet be destined to have large practical applications in engineering.

The process of anastatic printing, which was also the joint invention of William and his brother Werner, belongs to this period. As an evidence of the importance which was early accorded to this invention, it may be stated that it was considered worthy of being made the subject of a Friday evening lecture by Faraday at the Royal Institution in 1845. Siemens himself, in a lecture delivered by him at Birmingham on 20th October, 1881, referring to this invention, states that the favourable notice taken of it by Faraday "obtained for me an entry into scientific circles, and helped to sustain me in difficulty."

Another of William Siemens' early inventions was his water-meter, which exactly met an important practical requirement, and has had a splendid thirty years' success. It applied curiously subtle hydraulic principles, which, even irrespectively of the practical value of the instrument, are of great interest. Imagine a Barker's mill running absolutely unresisted. The discharged water must have approximately zero absolute velocity on leaving the nozzles; in other words, its velocity relatively to the nozzles must be approximately equal to the contrary absolute velocity of the nozzles. Hence the machine will rotate in simple proportion to the quantity of water passing through it. By an extension of similar considerations, it is easy to prove that if the wheel, instead of being unresisted, is resisted by a force exactly proportional to the square of its angular velocity, its velocity must still be proportional to the quantity of water passing through it per unit of time. Thus, provided this law of resistance is maintained, the whole angle turned through by the wheel measures the whole quantity of water that has passed. Now think of the difficulties which Siemens had to overcome to apply this principle. What has been roughly called a Barker's mill, must be completely inclosed in the supply water-pipes, its nozzles discharging into water, not into air. It must be of very small dimensions to be convenient for practice, and its bearings must be kept oiled to secure, not only that it may not be injured by the wear of running for years, but also that the constant frictional force of solid rubbing on solid may be as nothing compared to the resistance, proportional to the square of the velocity, exerted by the circumambient liquid upon a wheel with sharp edged vanes rotating in it. After a few years of trials, difficulty after difficulty was overcome, and the instrument did its work with the accuracy and convenience which met practical requirements. It was we believe the protection offered by the British Patent Law which, in the case of this very instrument, allowed Siemens to work it out in England, and so helped him eventually to find his home among us,

and to give us primarily the benefit of his great inventiveness in all directions; while the want of similar protection under German law at that time rendered it practically impossible for him to work out so difficult an invention in his own country.

In the foregoing we have selected those results of Siemens' inventive genius, not only on account of their position in time, or of the principles involved, but also, and perhaps mainly, because of the circumstances connected with them forming determining factors of such moment in his career. There is much in the lifework of such a man as Siemens, which while it might prove of great interest, and have some measure of importance in its bearing on his career as a whole, must yet of necessity be passed over in such a notice as this. We shall, therefore, here merely remark that during the ten years from his arrival in this country till about 1856, Siemens was employed on various engineering works. He was for some time engaged in railway work, and again in carrying out improvements at the famous calico printing works of Mr. Hoyle at Manchester.

William Siemens had early taken up the then new science of thermo-dynamics, and in 1847 he had already begun work on the regenerative principle—with which his name has since become indissolubly connected—in the employment of heat for the purpose of obtaining increased economy in its practical use. In that year he fitted up a regenerative steam-engine in the factory of Mr. Hicks at Bolton, and although the difficulties attending the use of superheated steam prevented the practical application of the invention, the importance of the principle embodied in it was recognised by the Society of Arts, who in 1850 awarded to William Siemens a gold medal "for his regenerative condenser." From papers contributed by him to various societies about this time—for instance, "On a New Regenerative Condenser as applied to High and Low Pressure Steam-engines," to the Institution of Mechanical Engineers in 1851; "On the Expansion of Isolated Steam, and the Total Heat of Steam," to the Journal of the Franklin Institute in 1852; and a most important paper "On the Conversion of Heat into Mechanical Effect," to the Institute of Civil Engineers in 1853 (for which he obtained the Telford Medal)—we see how persistently he clung to the problem of the better utilisation of heat. It was, indeed, at that time a conviction firmly fixed in his mind, that the various existing modes of using heat were wasteful and extravagant in the extreme, and he was continuously directing all his energies, not only to overcome the evil, but to convince others of its existence. The problem had always a fascination for him, and to the very latest pressed its claims upon his mind.

The great movement for smoke abatement, which has arisen within recent years, had his deepest sympathy, and his earnest labours on

its behalf during the last three years of his life may yet we hope have full effect. Just as those other wasteful modes of using heat yielded, as we shall see later, to Siemens' inventive genius persistently applied, so also, had he been spared to us, would this particular mode have had to yield in time.

Just nine days before his death, the writer of this notice had received a letter from Sir William Siemens, saying nothing of illness, but full of plans for the immediate future: chiefly an address to the Society of Arts, and the realisation at Sherwood of his method for the smokeless supply of heat to a steam-boiler, by the combustion of hydrogen, carburetted hydrogen, and carbonic oxide, obtained from the conversion into these gases of the whole combustible material of the coal, together with some hydrogen and oxygen from water, and oxygen from air, in his gas-producing kiln. "The producer will be in full operation at Sherwood by that time," were almost the last words received by the writer from his friend, kindly inviting him to come and see the new method in operation at the end of the current month. A short time before, in travelling home from Vienna, where they had been associated in the British Commission for the Electrical Exhibition, Sir William Siemens had told the writer that without waiting for a perfected gas-engine to use the products of combustion as direct motive agent, and so give the very highest attainable economy, he expected by using the gas from his producer as fuel for the fire of a steam-boiler, even on a comparatively small scale, like that of his appliances at Sherwood for electric lighting and the electric transmission of power, to be able to obtain better economy of coal for motive power than by burning the coal directly in the usual manner in a furnace under the boiler. And further, what is especially interesting to persons planning isolated installations for electric light, that he (Siemens) believed that the labour of tending the producer and boiler and steam-engine, would be on the whole considerably less than that which is required on the ordinary plan, with its incessant stoking of coal into the furnace under the boiler, as long as steam is to be kept up. There is something inexpressibly sad, even in respect to a comparatively small matter like this, to see the active prosecution of an experiment so full of interest and so near to a practical solution suddenly cut short by death.

In 1857 William Siemens, in conjunction with his younger brother and pupil, Frederick, commenced investigations with a view to using the principle of "regeneration" in the process of metallurgy. The result of their labours, in which it may be observed the elder brother had the greater share, was the regenerative gas furnace, certainly the most valuable and important of the many products of their genius which the Siemenses have given to the world. It was first practically applied, in 1861, within the works of the Messrs. Chance, of

Birmingham, to one of their glass-smelting furnaces, and proved a complete and undoubted success. As showing the saving to this industry alone, it may be observed that glass-smelting furnaces upon Siemens' principle are now in operation in which the fuel consumed is only 15 cwts. for every ton of glass produced, instead of 2 or 3 tons of fuel as was formerly the usual consumption for each ton of glass.

The great economy of fuel effected by the regenerative gas furnace, together with the great ease with which its high temperature could be maintained and regulated, soon led to its application in the production of iron and steel. In 1862 a paper by Siemens appeared in the "Chemical News" "On the Regenerative Gas Furnace as applied to Glass Furnaces, Puddling, Heating, &c.," and from that year the application of the regenerative principle to the processes of iron and steel making gained increasing attention from those concerned, having been very early adopted in the iron district of Lanarkshire in Scotland.

But Siemens did not rest content with the application of his regenerative gas furnace to the then known methods of iron and steel-making. With this new appliance he now turned his attention to the improvement of the methods of manufacture. His first attempt was in the production of steel on the open hearth according to the well-known experiments of Reamur, and in 1862 a practical trial of the arrangements devised by him to effect this was made at Tow Law by Mr. Charles Atwood, under a licence from William Siemens. This arrangement was found, however, to be faulty, and had to be laid aside. After one or two other disappointments, Siemens took the matter into his own hands, and in the famous sample steel works which he founded at Birmingham in 1865, he wrought out to a thorough practical success the process of steel making known as the Siemens Process, a subsequent improvement of which is the now well-known Siemens-Martin Process. Within three or four years after this the Landore Works, near Swansea, were organised on an extensive scale for the carrying out of this manufacture, William Siemens being here associated with Mr. Dillwyn, M.P., as the principal owners. The new material—mild steel—thus introduced, speedily proved its surpassing excellence for metallic structures in which strength and lightness were desired: while its manipulation was less difficult than that of iron on account of its greater ductility. Hence it at once began to displace iron in the construction of the highest class of steam-ships and marine steam-boilers, whilst of late years it seemed destined to altogether supplant iron as a material for the purposes of marine architecture and engineering. As an evidence of the extent to which these Siemens and Siemens-Martin Processes have been adopted, it may be

observed that amongst the very many licences which have been granted by the patentee, several of them have comprised hundreds of furnaces for various purposes. The total production of Siemens mild steel was over 340,000 tons in 1881 for Great Britain alone, while the Siemens furnaces in use here are approximately only one-third of those already set up in America and on the Continent.

The great things done by Siemens with gas produced in the manner referred to above, first in the gas glass furnace, described with glowing admiration by Faraday on Friday evening, June 20, 1862, in his last Royal Institution lecture, and more recently in connexion with the great and exceedingly valuable invention just referred to,—the Siemens process for making steel, by using the oxygen of iron ore to burn out part of the carbon from cast iron,—and still more recently in the heating of the retorts for the production of ordinary lighting gas, by which a large increase has been obtained in the yield of gas per ton of coal used, are achieved results which live after the inventor has gone, and which, it is to be hoped, will give encouragement to push farther and farther on in practical realisation of the benefits to the world from the legacy of his great inventions.

For his contributions in this department, towards the better application of science to the methods and processes of practice, William Siemens received many rewards and honours.

He received prize medals at the Exhibitions of 1851 and 1862, and a Grand Prix at the French Exhibition of 1867 for his regenerative gas furnace and steel processes. In 1874 he was presented with the "Royal Albert Medal," and in 1875 with the "Bessemer Medal" on account of his scientific researches and his inventions relating to heat and metallurgy, whilst only last week the Council of the Institution of Civil Engineers awarded him the Howard Quinquennial Prize for the advances he had made in the manufacture of iron and steel.

William Siemens has designated electricity "the youngest form of energy with which we are practically acquainted," and there is no doubt that to himself is due, in considerable degree, the great progress which has within recent years been made in its practical adaptation to various purposes.

It was in the early days of submarine telegraphy that Siemens entered the field of electric engineering, when the possibilities of ocean telegraphy were just coming into view, and it was therefore to this particular adaptation of electricity that he first applied himself. He took part in several of the early expeditions for cable laying in the Mediterranean and the Black Sea, acquiring there the practical experience, which helped him, in after years, in the arrangement and equipment of so many successful expeditions for a like purpose.

In 1858 the extensive telegraph works at Woolwich, now known as

Siemens Bros., were established by William Siemens in conjunction with Mr. Halske of Berlin, and the great undertaking so successfully carried out by this firm have more than a commercial interest. The scientific attainments, together with the wide experience and great inventive genius brought to bear by Siemens on the work entrusted to it, speedily succeeded in raising the firm to that eminence which it has ever since held. Alike in land as in sea telegraphic engineering, it has long held a first place, and while it would be out of place here to catalogue the various successes in cable laying and repairing of the Siemens firm, we may note that these include the North China, and Platino-Braziliera cables, the cables for the Indo-European lines, and no less than four Atlantic cables completed, and two new ones in progress at the time of Siemens' death. Many of the machines and processes now in use in the manufacture and maintenance of land and sea telegraph cables have been greatly improved, while some of them indeed owe their existence to William Siemens. One of these machines, the "Faraday," which is primarily a cable-laying and lifting machine, is a perfect monument of Siemens' practical genius. This vessel was designed by him in 1874 for the purpose of laying the Direct United States Cables, and was named the "Faraday" in grateful remembrance of that "master in science" to whom he owed so much.

It is remarkable that a ship capable of doing what no other ship afloat can do in the way of manœuvre, as has been proved by her success in the difficult and delicate operations of laying and lifting cables in depths of 2,500 fathoms, and of cable repairing in all seasons and all weathers, should have been the work of a landsman, born in the middle of Europe, who *early* made himself a sailor in cable-laying expeditions in the Mediterranean and the Black Sea, but whose life has been chiefly devoted to land engineering and science.

In the most recent advances that have been made in the application of electrical energy, William Siemens has been a pioneer, and it is in connexion with these that his name has become a household word. In the numerous and important inventions pertaining to this department of electrical engineering, to which the name applies. William Siemens has been associated with his brother Werner, and the world has profited largely by this brotherly co-operation of genius. More than a quarter of a century ago they brought out what is now known as the Siemens armature, which was shown at the London Exhibition of 1862, mounted between the poles of a multiple steel horseshoe magnet, and serving for the transmitter in an electric telegraph. That was what we may now call the one-coil Siemens armature. It suggested inevitably the mounting of two or more coils on the same iron core, in meridional planes at equal angles round the axis, and as nearly equal and similar in all respects as is allowed by the exigencies of completing the circuits with the

different portions of wire laid over one another, and bent to one side or the other, to avoid passing through the space occupied by the bearing shaft. The principle of electro-magnetic augmentation and maintenance of a current without the aid of steel or other permanent magnets, invented by Werner Siemens, and also independently by Wheatstone and S. A. Varley, was communicated to the Royal Society by William Siemens on February 14, 1867, in his celebrated paper on the "Conversion of Dynamical into Electric Force without the aid of Permanent Magnets." This paper is peculiarly interesting, as being the first scientific enunciation of that wonderful electro-magnetic principle, on which are founded the dynamo-electric machines of the present day. Soon after came the Paccinotti-Gramme ring, from which followed naturally the suggestion of the mode of connexion between the coils of a multiple-coil Siemens armature, described in the Siemens-Alteneck patent of 1873, and made the foundation of the Siemens dynamo as we now have it, whether as constructed by the Siemens firm, or with the modifications of details and proportions, valuable for many practical purposes, which have been contributed by Edison and Hopkinson. The evolution of the Siemens armature, as we now have it, in this splendid machine, from the rudimentary type which the writer saw a quarter of a century ago, is one of the most beautiful products of development by inventive genius, and is more like to the growth of a flower than to almost anything in the way of mechanism made by man.

It is unnecessary here to enter upon the consideration of the many new forms of apparatus for electric lighting given to the public by the Siemens firm, but we may perhaps mention the highly successful Siemens arc-lamp, and the various measuring instruments, particularly the Watt meter; this instrument being the realisation of a mode of estimating electrical energy or activity by a new unit—the "Watt"—suggested by Siemens in his Presidential Address to the British Association at Southampton in 1882. He at the same time suggested the "Joule" as a unit of heat or work, both of which units were at once adopted by electrical engineers as convenient practical units.

Just two months before his death Siemens witnessed the successful completion and formal opening for traffic of the Portrush and Bushmills electric tramway, one of the most splendid and interesting of his achievements. This railway, which is situated in the north of Ireland, and was the first of its kind in this country, now carries passengers on a $6\frac{1}{2}$-miles line of steep gradients and sharp curves, at a good 10 miles an hour, solely by water-power of the River Bush, driving, through turbines, a 250-volt Siemens dynamo at a distance of $7\frac{1}{2}$ miles from the Portrush end of the line.

We have seen how much the labours of William Siemens have contributed to make electricity subservient to the wants of mankind,

but, to show what to his mind were the possibilities of its future, we need only refer to the multifarious purposes to which electricity was applied at his country residence near Tunbridge Wells. There electricity might be found doing the great part of the actual work of the farm—sawing wood, and pumping water, and driving most of the various mechanical appliances on the premises. The very latest use to which it was here applied was for horticultural purposes. The electric light was used during the hours of darkness to continue the sun's work, in promoting the growth and ripening of plants and fruits, or, in other experiments, to compete with the sun in this work; the results in this latest application being made the subject of papers by Siemens, contributed to the Royal Society in March, 1880, and to the York Meeting of the British Association in 1881.

In the foregoing, we have sought to exhibit the life work of Sir William Siemens, not in its chronological order, but as viewed in the two departments "Utilisation of Heat" and "Utilisation of Electricity." Of the thirty-five papers which appear under his name in the Catalogue of Scientific Papers, and which do not include the last ten years of his life, all are with, perhaps, one not happy exception, more or less directly the result of his efforts in one or other of these directions. The exception referred to is his paper "On the Conservation of Solar Energy," contributed to the Royal Society, February 1882, and embodied in an article "A New Theory of the Sun," published in the "Nineteenth Century" of April 1882; but even this may well be considered no exception, from its evident connexion with the principle of "Regeneration of Heat," and treating, as it does, of the sun imagined as a great regenerative gas furnace. Careful consideration of the circumstances, however, shows this view to be wholly untenable. While his papers are thus almost wholly of a practical nature, his work in the domain of pure science has been neither slight nor unimportant, because, in experimentally developing his inventions, his mind was ever on the alert, and hence his efforts towards the practical application of the results of science, in many cases, served to put these results in a clearer light.

Sir William Siemens has received recognition of his services to pure and applied science from the Emperor of Brazil, the Shah of Persia, and from France both under the Empire and the Republic, whilst in April last Her Majesty was graciously pleased to confer upon him the honour of knighthood. He was a member of nearly all the scientific societies of Great Britain; he was the senior member of council of the Institution of Civil Engineers; he was elected a member of the Royal Society in 1862, and has twice served on the council of that body. He has been President of the Institution of Mechanical Engineers, twice of the Society of Telegraph Engineers, of the Iron and Steel Institute, and last year, at Southampton, of the

British Association; whilst at the time of his death he was Chairman of the Council of the Society of Arts. He was made a D.C.L. of Oxford *honoris causâ* in 1870, an LL.D. of Glasgow in 1880, of Dublin in 1882, in which year the University of Würtzburg also bestowed on him its honorary Ph.D. He was a corresponding or ordinary member of several learned societies in Europe and in America.

In private life Sir William Siemens, with his lively, bright intelligence always present, and eager to give pleasure and benefit to those around him, was a most lovable man, singularly unselfish and full of kind thought and care for others.

We shall conclude by quoting the closing lines of an article which appeared in the "Times" of November 21, 1883, "On the Life and Work of Sir William Siemens." They are words with which all who knew him or came in contact with him can fully sympathise, and they will, besides, serve to indicate the estimation in which he was held by the people of his adopted country:—"Those who knew him may mourn the kindly heart, the generous noble nature, so tolerant of imperfect knowledge, so impatient only at charlatanism and dishonesty; the nation at large has lost a faithful servant, chief among those who live only to better the life of their fellow-men by subduing the forces of nature to their use. Looking back along the line of England's scientific worthies, there are few who have served the people better than this her adopted son—few, if any, whose life's record will show so long a list of useful labours."

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Jean Baptiste André Dumas was born at Alais, in the Department of the Gard, July 14, 1800.

The little town of Alais was almost unknown at the beginning of this century, being inhabited by only a few thousand souls. Nevertheless, young Dumas found there everything conducive to the expansion of a youthful intellect and to the development of a well-built frame.

A college which had then no lack of pupils fulfilled the requirements of the boy's early education, initiating him more especially in the study of Latin, so congenial to the classical traditions of the neighbourhood.

These associations could not but have had a tendency to direct the mind of young Dumas to the study of the past; but there were other influences, not less potent, continually calling him back to the present. Indeed, the town of Alais, by its unique situation, afforded opportunities of observing nature and the processes of adapting her products to the use of man, which proved not less attractive to the future Academician. Both in his speeches and writings he frequently refers with gratitude to these varied impressions of his early youth at Alais.

It would be difficult to imagine a happier complement to a classical education than the lessons taught at every step in this delightful country. Nor were they lost upon young Dumas, who at fourteen years of age, in addition to his rare attainments in classical literature, had acquired a rudimentary knowledge of the several natural sciences. Having made up his mind to enter the navy, he might at once have presented himself for examination, had it not been for an insufficient acquaintance with some branches of mathematics, in which, owing to the limited instruction given at the college, his information had hitherto remained of a very elementary character.

While Dumas was still preparing for his naval examination, the political events of 1814-15 obliged his family to renounce this project and to select a career for the youth which would entail less sacrifice.

Dumas accordingly entered as apprentice at an apothecary's in Alais. This position, in which he pursued his first practical studies, did not afford much opportunity for scientific progress, and the young man became soon impressed with a strong desire to give up this place and to quit his native town. This feeling, indeed, became so intense that his parents, moved by his evident distress, thought it best to accede to his wishes.

Soon after, in 1816, Dumas travelled on foot from Alais to Geneva, where he found everything to expand his ideas, to stimulate his emulation, and thus to prepare him for his future career. There were lectures on botany by M. de Candolle, on physics by M. Pictet, and on chemistry by M. Gaspard de la Rive. He had, besides, the superintendence of a tolerably large laboratory, belonging to the pharmacy of Le Royer, and formerly used for the courses of applied chemistry given by M. Tingry.

The pharmaceutical students, who frequently united in botanical excursions during the summer, started the idea of winter meetings for scientific studies. Seeing that Dumas had a laboratory at his disposal, it was suggested that he should give them a course of experimental chemistry. This was his *début* in the professorial career.

Meanwhile Dumas had become introduced to Gaspard de la Rive, to Theodore de Saussure, and to De Candolle, and each of these in his way began to take a warm and lasting interest in him; they encouraged his studies, and assisted him to the best of their powers in his pursuits. It was most likely at the instigation of his new friends that Dumas, reviving his early naval predilections, began seriously to think of, and to prepare for, an exploring expedition to some distant part of the world. A monograph on the Gentianeæ, chiefly written for the purpose of becoming familiar with the language and the ideas of botanical science, was a fruit of these aspirations.

But this was not to be his mission. Biot's great treatise, which for

half a century was to remain the standard text-book on physics, had just appeared; and Dumas had found, more especially in the first volume, plenty of subjects directing his attention to the art of experimenting, of making observations, of consulting nature, and of discovering the laws of her phenomena. The "Annales de Chimie" offered him, moreover, splendid models in the papers of Berzelius, Davy, Gay-Lussac, and Thénard. At the same time he studied with indefatigable zeal the works of Lavoisier, and the "Statique Chimique" of Berthollet.

Dumas was then eighteen years of age. It was about that time that he had the good fortune to make himself useful to one of the principal physicians of the town, a circumstance which contributed not a little to advance him in the circles in which he had hitherto lived. Dr. Coindet asked him to examine some carbonised sponges, and to ascertain more especially whether iodine was present in them. Having after some days received an affirmative answer, Dr. Coindet no longer hesitated to consider iodine a specific against goitre. Dumas was then asked to give his attention to the subject and to point out the preparations in which iodine might be most conveniently administered. He suggested tincture of iodine, potassic iodide, and iodised potassic iodide. Soon afterwards these new remedies were mentioned in a German journal published at Zurich, and it is in this connexion that the name of Dumas is first met with in scientific literature.

About that time Dr. J. L. Prévost, after an absence of several years, returned to Geneva. He had long been resident in Edinburgh and Dublin, devoting himself to comprehensive studies in the several departments of medicine. Among these was a particular examination of the physiological effects of digitalis, and he was naturally anxious to obtain the active principle of the plant free from all foreign matter accompanying it. He invited Dumas to join him in this inquiry. The problem to be solved consisted in successively removing all that appeared inert so as to concentrate the active constituent, which would ultimately remain in a state of purity. The chemical properties of this principle being unknown, the only means of estimating the concentration was to observe the effect of the concentrated substance upon animals. This slow and irksome process of elimination did not lead to any result; it is well known that the isolation of digitaline was not effected until some years later. But however unsuccessful, these joint labours gave rise to a far more fruitful collaboration.

Whilst studying together the physiology of Richerand, a work then in great repute, and the memoirs of Magendie, which were beginning to attract increased attention, the two friends resolved to engage in a series of chemico-physiological researches.

It seemed natural enough to commence these researches by a

renewed study of the blood, and they were soon able to publish a paper upon this subject. This appeared in the "Bibliothèque Universelle de Genève," and in the title Dumas still figures as *Elève de Pharmacie.* The results arrived at by the young inquirers for a long time satisfied the wants of science, and if our knowledge of the blood has been considerably expanded by many subsequent observers, the experiments of Prévost and Dumas have invariably served as a starting point for these inquiries.

It was about that time that the death of Princess Charlotte had excited a feeling of sorrow all over Europe. The pathological problem presented by this sad event induced the two experimentalists to resume the study of the transfusion of blood. This problem has since been frequently examined, but these renewed inquiries have added but little to the knowledge elicited by Prévost and Dumas' researches upon the subject.

A very important result at which they arrived was, moreover, the demonstration of the presence of urea in the blood of animals the kidneys of which had been removed. They inferred from this observation that the function of the kidneys is not to produce but to eliminate urea formed in the blood. Their experiments have been repeated by the most distinguished observers, among whom it suffices to name Gmelin, Tiedemann, and Mitscherlich, and the conclusions to which they led are therefore generally adopted by physiologists. We should not, however, leave it unmentioned that, during the last few years, some dissenting voices have been heard, and that some modern physiologists believe they have proved that urea is also generated by the kidneys. But if it should ultimately be proved, beyond doubt, that the kidneys, like many other organs, more especially the liver, have the power of producing urea, elimination of urea formed elsewhere would be still the principal function of the kidneys, although not the only one, as Prévost and Dumas have been led to believe from their experiments.

From the researches on blood Prévost and Dumas proceeded to the examination of the phenomenon of fecundation, the knowledge of which they considerably expanded. It deserves more especially to be noticed that, notwithstanding some previous observations by Swammerdam and Spallanzani upon the subject, modern physiologists are unanimous in recognising Prévost and Dumas as the discoverers of the phenomenon of segmentation in the ovum of the Batrachians. At the same time these investigators observed that at a certain stage of fecundation there escapes from the ovary of the Mammalia a limpid almost microscopic vesicle which enters the Fallopian tube, and proceeds to the uterus, where, when impregnated by the spermatozoids of the male, it is fixed, and, increasing in size and development, gives rise to the fœtus. Prévost and Dumas must

thus be looked upon as the precursors of C. E. Baer, whose classical researches on the genesis of the ovum in the Mammalia and in man appeared in 1827.

Simultaneously with their researches on blood and fecundation, Prévost and Dumas published several other physiological investigations not immediately connected with the main subjects of their studies. The urine and the organs of secretion of frogs successively engaged their attention. They also studied the phenomena accompanying the contraction of muscular fibre.

Lastly, Prévost and Dumas' suggestion as to the treatment of stone by electricity should not be left unnoticed. Their experiments showed that the current of a powerful battery is capable of destroying and dissolving the phosphatic calculi of the bladder, without its mucous membrane being materially affected. Although, at a subsequent period, these researches were continued and materially enlarged by the late Dr. Bence Jones, the author of this sketch has not been able to learn that the treatment indicated by these remarkable results has been successfully applied in surgery.

At this period, *i.e.*, in 1822, Dumas might have settled at Geneva, and many circumstances led him to think seriously of doing so. An incident, however, which happened at that time, and which at first sight seemed in no way likely to influence a well-matured plan of life, induced him within a few days to change his mind. He made the acquaintance of Alexander von Humboldt, who invited him to be his companion during a few days' stay at Geneva. The short intercourse with this extraordinary man suddenly expanded his aspirations.

The memorable hours he had spent with him had opened a new world to his mind. He had been more especially impressed with what he had told him of Parisian life, of the happy collaboration of men of science, and of the unlimited facilities which the French capital offered to young men wishing to devote themselves to scientific pursuits. He began to think that Paris was the only place where under the auspices of the leaders of physical and chemical science, with whom, he had no doubt, he should soon become acquainted, he might hope to find the advice and assistance which would enable him to carry out the labours over which he had been pondering for some time. His mind was soon made up—he must go to Paris.

Dumas' removal to Paris, which took place in 1823, brought the physiological labours in which he had been engaged along with Prévost to a conclusion. Though the separation from a friend, with whom he had been in daily intercourse for so many years, must have been deeply felt by the young *savant*, who had now to steer his course alone, he had the good fortune to become acquainted with three young men of about his own age, with whom he soon entered into friendly

alliance. These were Victor Audouin, the zoologist, well-known even at that time, Adolphe Brongniart, who had already published several important botanical papers, and Henri Milne Edwards, who had just terminated his medical studies and was working for his degree. The friendship of these three men, matured by daily intercourse and subsequently strengthened, if possible, by family ties, has ever been looked upon by Dumas as one of the most important acquisitions of his life, not only proving to him an inexhaustible source of the purest pleasures, but likewise materially assisting in shaping that successful career which has made the name of Dumas a household word in the mouths of chemists.

If a legitimate desire to become acquainted with the leading men of science of that day was one of the principal motives in determining Dumas to leave Geneva, his wishes were gratified far beyond his most sanguine expectations. Nothing could have surpassed the kindness with which the young aspirant was received by the very men to whom he had hitherto been looking up with mingled sentiments of reverence and awe. Indeed, a most kindly feeling of good fellowship towards youthful workers in the same field of inquiry was a noble feature in the character of nearly all men of science of that period. La Place, Berthollet, Vauquelin, Gay-Lussac, Thénard, Alexandre Brongniart, Cuvier, Geoffroy Saint Hilaire, Arago, Ampère, Poisson, all gave striking proofs of their desire to smooth the path of young investigators, and thus to promote the advance of science.

The place of Répétiteur de Chimie to Thénard's course of lectures in the École Polytechnique having become vacant at that time, Arago proposed Dumas for the office, and he was elected by the council of the school before he had become aware that he was a candidate. There was at that period in Paris an establishment for evening lectures on literature and science, resembling in a measure the Royal Institution of Albemarle Street, though the literary element predominated. The chair of chemistry at that institution, often called Lyceum, but better known by its later name of Athenæum, had become vacant, and Ampère succeeded in procuring the appointment for Dumas without having previously spoken to him on the subject.

From this moment, owing to the influence of his two illustrious protectors, the study of physiological questions receded more and more in the background, while his full energy was directed towards the solution of chemical problems.

Still many circumstances conspired which prevented Dumas from engaging much in scientific researches during the first years of his stay in Paris. His lectures at the Athenæum required a great deal of preparation; he was, moreover, in his capacity as assistant to Thénard's course at the École Polytechnique, assiduously practising the art of experimenting in public, in which he soon attained the

highest degree of proficiency. At the same time he founded (in 1824), with his friends Audouin and Brongniart, the "Annales des Sciences Naturelles," and also began to collect the materials necessary for the publication of his grand "Traité de Chimie appliquée aux Arts," the first volume of which appeared in 1828.

But if this period was for Dumas one of incessant labour, and often of the most strenuous efforts, it also enabled him to realise the most ardent of his aspirations. For some time Dumas had been intimate with the family of Alexandre Brongniart, the father of his friend Adolphe, and was not long before he became betrothed to Mdlle. Herminie Brongniart, the eldest daughter of the illustrious geologist. It was on February 18, 1826, that the matrimonial alliance was concluded which has been for more than half a century a source of the purest happiness for both consorts.

At the very commencement of his labours in the cause of organic chemistry Dumas had found himself face to face with a powerful rival in Germany, who, setting out by a curious coincidence from the same starting-point, the study of pharmacy, had entered the lists without passing through the physiological and natural history stages of his competitor. Liebig and Dumas have had, indeed, some strange encounters on the field of science. These encounters, to which we may have to refer hereafter, were occasionally rather violent, as might have been expected when two young and fiery champions, each persuaded of the justice of his views, were rushing at each other. Once or twice perhaps in the heat of battle an unguarded word might have sounded like personal provocation; but however fierce the aggression, the combatants never forgot that they were both fighting under the banner of truth, and the contest having been decided, the antagonists invariably separated with increased regard for each other.

But we must return to Dumas' early labours in the field of experimental investigation. They were by no means exclusively devoted to organic chemistry; indeed, one of his first researches, which riveted at once the eyes of the scientific world upon the young French chemist, was of a much larger scope. We allude to the classical paper, "On some Points of the Atomic Theory," which was published in the "Annales de Chimie et de Physique" for 1826, and in which the author soars to the very heights of chemical science. Whoever to-day, after a lapse of nearly sixty years, peruses this admirable memoir, which aims at the solution of old problems by new methods, cannot but gratefully acknowledge that a good deal of what has long since become common property, is rooted in its substance; but he will at the same time be astonished to perceive that many of our modern views, which we are in the habit of considering as the acquirement of the last few decades, had already found expression in this paper.

In glancing back at the results of this memoir in the light of our

present views, we perceive at once what a start the French chemist had gained on his contemporaries. "I am engaged in a series of experiments," he says, "intended to fix the atomic weights of a considerable number of bodies, by determining their density in the state of gas or vapour. There remains in this case but one hypothesis to be made, which is accepted by all physicists. It consists in supposing that in all elastic fluids observed under the same conditions, the molecules are placed at equal distances, *i.e.*, that they are present in them in equal numbers."

It is obvious that the author opens his inquiry with the very conceptions which form the basis of our present views in chemical philosophy; and it is only to be wondered at that the happy enlistment of Avogadro's ideas into the service of chemistry, which we owe to Dumas' initiatory sagacity, should for more than a quarter of a century almost have fallen into oblivion.

Having premised in lucid terms the general scope of his inquiry, Dumas proceeds to describe the several modifications of the well-known method of taking vapour-densities with which he has endowed science, and which went forth from his hands in such a state of consummate perfection that there has scarcely been room left for subsequent emendation.

The narrow compass of this sketch does not permit us to quote the numerous results communicated by Dumas in his paper; we will mention, however, that the determination of the vapour-density of chloride and fluoride of silicon elicited the view now held regarding the constitution of silicic acid, thus overthrowing the old formula of this compound upon which Berzelius had based his classification of silicious minerals.

There were other experimental researches of importance carried out by Dumas about this period. It had long been his intention to resume the study of the compound ethers, to which he had devoted considerable attention at Geneva. In conjunction with Boullay he proceeded to an elaborate investigation of nitrous, acetic, benzoic, and oxalic ethers. The composition of these substances was finally settled by accurate combustions and vapour-density determinations. They further elicited by unequivocal experiments the capital fact that the decomposition of compound ethers by alkalies gives rise to quantities of acids and alcohol, the joint weight of which is greater than the weight of the compound ethers submitted to experiment, and by accurately determining this difference they succeeded for the first time in establishing the nature of compound ethers on the solid foundation of experiment.

These investigations led to other inquiries teeming with most remarkable results, among which, those on the formation of oxamide, on chlorocarbonic ether, and on urethane may be specially

mentioned. Here, also, the splendid papers on wood-spirit and spermaceti, jointly published by Dumas and Peligot, deserve to be noticed, although they belong to a somewhat later period.

While the experiments on the ethers sketched in the preceding paragraphs were still going on, a strange incident directed the attention of Dumas to a perfectly different order of phenomena, the study of which occupied him for many years of his life, and ultimately led him to one of his finest conceptions, the theory of substitution. It is not generally known that it is to a *soirée* at the Tuileries that the origin of the substitution-theory must be traced. One evening the visitors at the palace were greatly incommoded by irritating vapours diffused throughout the apartments, and obviously arising from the wax candles burning with a smoky flame. Alexandre Brongniart, in his capacity of director of the porcelain manufactory at Sèvres, was looked upon as chemist to the king's household, and it appeared but natural that he should be consulted respecting this unpleasant incident. Brongniart intrusted his son-in-law with the task of investigating the suspicious candles, and Dumas was all the more inclined to engage in this inquiry, that he had already made some experiments in that direction, having been asked by a merchant to suggest a method of bleaching certain kinds of wax which resisted the ordinary processes, and thus remained unsaleable. Nor had Dumas any difficulty in supplying the explanation. The irritating vapours were chlorhydric acid, and it was thus obvious that the candle manufacturer supplying the palace had made use of wax bleached with chlorine, and that the chlorine-bleached wax had retained chlorine, which during the combustion of the wax was evolved in the form of chlorhydric acid. The origin of the inconvenience experienced at Charles X's *soirée* was thus satisfactorily explained, and its recurrence easily obviated. At the same time it was proved by experiment that organic substances when treated with chlorine are capable of fixing this element in quantities far too large to admit the assumption of its presence being an accidental contamination. A new field of investigation was thus opened, on which within a comparatively short time a harvest of results of startling novelty was reaped by Dumas.

It is well known that the researches suggested by the experiment above mentioned led to the view of chlorine being capable of replacing hydrogen, atom for atom, in organic compounds.

This view, diametrically opposed to the dualistic conception of the electrochemical theory of the time, was vehemently contested by Berzelius and his school, who exhausted the resources of argument, scorn, and even ridicule against them. Nevertheless, Dumas' ideas rapidly took root, and but a few years later substitutional conceptions began to prevail in the researches of the younger generation of

chemists. An additional impetus was given to the movement when it was joined by Laurent, who, though often so much at variance with Dumas, as to become, more especially in consequence of questions of priority as to certain collateral ideas, his declared opponent, has nevertheless, by amplifying the original conceptions and by presenting in his unremitting researches ever new and welcome illustrations of them, assisted more perhaps than any other chemist in the propagation of the theory of substitution.

We cannot, of course, attempt to examine the several experimental researches, extending over a considerable space of time and embracing a great variety of subjects, which served as scaffolding to Dumas when building up his substitutional and typical conceptions; all we can do is to allude in a few words to the experiments which furnished him with his principal illustrations.

Proceeding chronologically, we should have to refer in the first place to Dumas' experiments on cinnamon oil and cinnamic acid. Again, excellent illustrations of substitution are furnished by his work on olefiant gas and ordinary ether.

His examination of the action of chlorine upon alcohol, too, served to illustrate his theory; although in these experiments he was forestalled by Liebig, who by his researches had been led to the discovery of chloral and chloroform. But if Dumas lost the discovery of these two compounds, he had at all events the satisfaction of establishing their true composition, and of thus supplying the key to the correct interpretation both of the formation of chloral from alcohol and of its decomposition, first pointed out by Liebig, into formic acid and chloroform, when submitted to the action of alkalies.

But the inquiry, which more perhaps than any other has contributed to establish Dumas' ideas in the minds of chemists, is his splendid investigation of the action of chlorine upon acetic acid. The trichloracetic acid formed in this reaction retains all the characteristic properties of the mother compound, its salts and its ethers resemble those of acetic acid, and when Berzelius and the champions of dualistic views still endeavoured, by constrained interpretation, to prove acetic and chloracetic acid to differ in constitution, Dumas showed that even their metamorphoses are strictly analogous.

Among the numerous researches undertaken with a view of elucidating the theory of substitution, a joint inquiry of Dumas and Stas on the action of alkalies on alcohols and ethers must not be forgotten. They prove that the alkalies act as oxidising agents, hydrogen being evolved, whilst the acids belonging to the alcohols are simultaneously produced. Amylic alcohol, then just brought to light by Dumas and Cahours' researches, is thus found to yield valeric acid, up to that time obtained only from *Valeriana officinalis*.

A few years later Dumas returned once more to the acids generated

by the oxidation of the alcohols. But what on this occasion fixed his attention was the simple relation in which these acids stand to each other. For the first time, indeed, we hear of the series of fatty or aliphatic acids. The observations recorded by Dumas have greatly contributed to develop the classification of organic compounds in homologous series. A very important series of this kind was indicated by Dumas when, on this occasion, he showed that between formic and margaric acids not less than fifteen acids could be assumed to exist, differing from one another by a constant elementary difference, CH_2, of which nine at least were known at that time.

The number of elements with which organic chemistry works in building up her structures being so very limited, it was but natural that from the very first considerable attention should have been bestowed upon the quantitative analysis of organic substances; and that we find the very chemists who laid the foundation of organic chemistry also engaged in elaborating the methods for determining the organic elements. There are, indeed, no two chemists to whom we are more deeply indebted for the growth of our methods of analysing organic substances than Liebig and Dumas, and we are delighted that in the language of the laboratory their names remain associated with the processes they have introduced. We speak of Liebig's method of estimating carbon and hydrogen, and of Dumas' process for determining nitrogen.

Referring to the methods of determining the composition of organic substances, it is but natural that we should allude to the services which Dumas has rendered to organic analysis, by the revision jointly carried out with Stas of the atomic weight of carbon. This revision furnished the number 12 instead of 12·24, which was the number adopted by Berzelius. Dumas and Stas's investigation will ever be looked upon as a model for experimental researches, and their atomic weight of carbon, although it has still suffered a trifling modification by subsequent researches, has since been universally adopted.

The results arrived at in this inquiry naturally also led to a revision of the atomic weight of oxygen—in other words, to a revision of the composition of water—which appeared all the more desirable, since chemists at that period very generally began to use the atomic weight of hydrogen as the atomic unit instead of that of oxygen which had been previously employed. Experiments made by Dumas on a scale not hitherto attempted, and consisting in the reduction of large quantities of oxide of copper—from 300 to 900 grams were used—and determining the oxygen supplied by the oxide reduced as well as the water formed, showed the volume weight of oxygen to be exactly 16, and thus the fundamental numbers 1, 12, and 16 for hydrogen, carbon, and oxygen were acquired which for a long time satisfied the wants of chemists.

The corrections to which the experiments previously mentioned had led, as regards the composition of carbonic acid and water, suggested also a re-examination of atmospheric air. Dumas undertook this investigation in conjunction with his friend Boussingault. The method of analysis adopted was exclusively ponderal.

The composition of air thus arrived at by Dumas and Boussingault is—

	By weight.		By volume.
Oxygen	23	....	20·81
Nitrogen....	77	....	79·19
	100	....	100·00

The rectification of the atomic weight of carbon and the inquiries more immediately connected therewith must be looked upon as the prelude of Dumas's long series of researches on the atomic weights of the elements. They were mostly published at a later period (from 1858 to 1860). A few fragmentary statements must suffice to convey to the reader an idea of the magnitude and variety of these researches. They embrace not less than thirty elements, or about one-half of those then known; the number of experiments made for the purpose of fixing their atomic weight closely approach 200, so that on an average about six separate analyses were made in each case.

Among the researches carried out by Dumas in conjunction with other chemists we have still to notice those with Malaguti and Leblanc on the transformation of the ammonic salts and amides into the alcohol cyanides (nitriles), those with Cahours on the composition of the neutral nitrogenous substances in the vegetal and animal organism, those with Milne Edwards on the conversion of sugar into fat (wax) within the organism of the bee.

The last experimental inquiries published by Dumas are his researches on alcoholic fermentation (1872), and an interesting paper on the occlusion of oxygen in silver, which appeared as late as 1878.

Lucidity of exposition and grace of style are not necessarily associated with the gift of successfully interrogating nature. It happens but too frequently that the results of admirable inquiries are almost hidden in papers hastily, not to say negligently, written. But no one ever found fault with Dumas in this respect. Few chemists, perhaps, published their researches in a more attractive and lucid form. And the same graceful elegance and perspicuity of style are found in whatever proceeded from his pen. One might fancy that he took the same pains whether it was a friendly letter or an elaborate report, a festal oration or a philosophic essay that he was writing; or, perhaps, we should rather say, they seem all to have been written with the same facility.

The works of Dumas present considerable variety, both as to the

subjects discussed and to the form of treatment adopted. There are several elaborate treatises and a great many minor pamphlets. His academical notices, his official documents, his municipal reports, his festal speeches, his opening discourses, his commemoration addresses, his funeral orations, are countless. We may be allowed briefly to allude to his more important writings.

Among these, his "Traité de Chimie appliquée aux Arts" deserves to be noticed first. This important work, which is dedicated to Baron Thénard, consists of eight volumes, the first of which, as has been already stated, appeared as far back as 1828; the last one was published twenty years later. It is accompanied by a fine Atlas of Plates. The treatise has been translated into several languages, the German edition being by Gottlieb Alexander and Friedrich Engelhart. In the preface the author informs us that the book is founded upon the notes collected for a course of chemical technology extending over three years, which he had to deliver at the Royal Athenæum. And nothing could give a better idea of the time and energy devoted to the preparation of these lectures. The labour of accumulating such a mass of facts must certainly have been enormous, nor could the effort made in disposing them in such luminous order have been less.

At a later period, about ten years after the first volume of the "Traité de Chimie appliquée aux Arts" had appeared, Dumas' celebrated "Leçons sur la Philosophie Chimique" were published. In these eleven lectures, which, during the summer of 1836, were delivered in the College of France, he traces the development of chemical doctrines from remotest antiquity up to the time at which the course was given. Indeed, the last lecture is devoted to the generation of electricity by chemical action, to the chemical effects of the battery, to the ever-memorable experiments of Sir Humphry Davy, and to thee lectro-chemical theory he founded thereon, as well as to the electro-chemical theories of Ampère and Berzelius; while it concludes with a survey of Faraday's electrolytical researches.

Among the numerous writings of Dumas none, perhaps, has found a more cordial reception in wide-spread circles than the lecture with which on August 23, 1841, he concluded his course of chemistry in the Medical School of Paris. This lecture is published under the title "Essai de Statique Chimique des Êtres Organisés," par MM. Dumas et Boussingault, and gives in a simple form the principal features of the life of plants and animals considered from a chemical point of view, presenting a most eloquent *résumé* of the chemical and physiological researches in which the two friends, either individually or jointly, had been engaged for many years.

The publication of this lecture gave rise to a dispute between Dumas and Liebig regarding the priority of the ideas propounded

therein. The great German chemist having but a year previously, in 1840, published his celebrated work, "Organic Chemistry in its Application to Agriculture and Physiology," had been naturally led to investigations of a similar order concerning the chemical phenomena of animal life, and was then actually preparing his "Chemistry applied to Animal Physiology." Liebig, no doubt, had freely stated the results of his researches in lectures delivered long before the publication of Dumas and Boussingault's pamphlet; but there is not a shadow of proof that Dumas was influenced by inquiries which at the time were not published. The accusations, it cannot be denied, rather hastily hazarded by Liebig, could not but cause a temporary estrangement between the two great chemists. Fortunately it was of only short duration, and left, as we already have had occasion to learn from their own mouths, no bitterness in their minds. Nor was there any cause for such estrangement. Indeed, the unbiassed reader of to-day no longer doubts that the conceptions which formed the subject of dispute were independently arrived at by both inquirers. And we are all the more confirmed in this view when we learn that documents have since been found which unmistakably prove that as far back as 1792 Lavoisier was acquainted with the mutual relation presented by the phenomena of vegetal and animal life.

We have still to allude to the important series of commemoration addresses which Dumas delivered on many of his departed friends and colleagues. Each one of these addresses, which collected would fill an imposing volume, is a work of art we are never tired of contemplating; each one attains its end by giving a life-like portrait of the person commemorated, a portrait which remains indelibly stamped on our memory. We know not which to admire most, the conciseness which excludes all that is non-essential from the sketch, or the poetic inspiration which fires the monumental style and throws upon the form it pictures the light of an ideal conception. Nor are these addresses wanting in numerous interesting particulars which, drawn from the author's own personal intercourse with his heroes, give a life-like colouring to his portraits. Such commemoration addresses Dumas has delivered on Auguste Béroet, Jules Pelouze, Geoffroy Saint-Hilaire, Auguste de la Rive, Alexandre and Adolphe Brongniart, Guizot, Antoine Balard, Count Rumford, Victor Regnault, Charles and Henri Sainte Claire-Deville.

Nor should, when Dumas's commemoration addresses are enumerated, his beautiful Faraday lecture be left unnoticed. It is well known that soon after Faraday's death in 1867 the Council of the Chemical Society of London organised a periodical celebration of his life and labours by instituting a triennial prize to be conferred upon scientific men of all countries whom they proposed from time to time to invite for the purpose of rendering homage to the memory of the

great experimental inquirer of our century. It was Dumas who, on June 17, 1869, opened this cycle of commemoration addresses by delivering a most eloquent lecture in the theatre of the Royal Institution, where the voice of Faraday himself had been so often heard.

Essentially different from these commemoration addresses, but not less masterly of their kind, are the numerous orations he delivered —sometimes in the name of the Academy, sometimes in his capacity as Vice-President of the Educational Council—at the funeral obsequies of distinguished men, amongst which those on Elie de Beaumont (1874), on Le Verrier (1877), and on Claude Bernard (1878), may be specially mentioned.

But there were other duties than the delivery of commemoration addresses in store for the academician. Any task imposed upon the Institute in the accomplishment of which chemistry was directly or indirectly concerned, invariably devolved upon Dumas.

The "Comptes Rendus" of the last fifty years contain an endless series of reports addressed to the Academy, which, on a great variety of subjects, either alone or in conjunction with some of his colleagues, he drew up. Were we to attempt to do full justice to this part of Dumas' work, we should have to ask the reader to accompany us into the most different departments of inquiry.

Some of these reports are elaborate essays, the interest in which will not die with the ephemeral conditions of their origin. Among them, those on Nicolas Leblanc and the early history of the soda-process, on the diseases of the silkworm, on the devastations of the phylloxera, may be quoted as illustrations.

We have still to allude to some literary achievements of another character. It has been already stated that in 1824 Dumas founded, in conjunction with his friends Audouin and Adolphe Brongniart, the "Annales des Sciences Naturelles." At a later period, in 1840, he became one of the editors of the "Annales de Chimie et de Physique," an office which he held up to his death. He has thus been an editor of that journal for upwards of forty years, but his contributions to it extend over more than half a century.

The lectures at the Athenæum, together with the literary engagements which they had occasioned, his duties as Répétiteur at the École Polytechnique, and the experimental researches continued without interruption, would have left but little leisure to any man of ordinary energy. Dumas, however, found time for additional work. Well aware of the imperfection of scientific instruction for technical purposes in the then existing institutions of France, he conceived the idea of establishing, in conjunction with his friends Théodore Olivier and Eugène Péclet, a school intended to supply the defect. The new school, which assumed the title of "École Centrale des Arts et Manufactures," was opened in 1829.

At first Dumas lectured at this school on general, analytical, and industrial chemistry. At a later period, when its financial position permitted the appointment of additional chemical teachers, he confined himself to either one or other of these branches. The lectures on general chemistry he continued up to 1852, when he resigned in favour of Cahours.

The number and variety of lectures which Dumas had to deliver at the École Centrale immediately after its opening, in addition to his duties at the Polytechnic School, rendered it an absolute necessity for him to diminish his engagements elsewhere, so as to enable him to find time for the various researches he had then in hand. Nor did he hesitate (in 1829) to retire from the Professorship at the Royal Athenæum, to which Bussy was then appointed. The alleviation thus obtained was not of long duration. In 1832 Guy-Lussac resigned his chair at the Sorbonne, which, like a natural inheritance, fell to Dumas; and to this position, which he held up to 1868, when Henri Ste. Claire-Deville, after having acted as his substitute since 1853, became his successor, was soon added another important appointment. For when (in 1835) Thénard withdrew from his Professorship at the École Polytechnique, the duties of the office devolved upon Dumas, who for twelve years had been a Répétiteur at the School. Dumas was, in fact, appointed, and remained in connexion with the Institution up to 1840, when he resigned in favour of Pelouze. The list of his Professorial appointments, however, is not yet exhausted. After the death of Deyeux (in 1839) he was induced, chiefly by Orfila, to undertake the duties of the Chair of Chemistry in the École de Médecine.

In these several positions Dumas had to lecture on very different subjects: he had, moreover, to shape his courses according to the traditions of the places in which he lectured, and to adjust them to the different ages, acquirements, and wants of the students he addressed. His unprecedented success as a lecturer is unequivocally proved by the lively and lasting recollections which his lectures, addressed to such a diversity of audiences, have left in the minds of his hearers. Even those who have had the good fortune of attending but a single one of his lectures will ever remember the clearness and precision of his reasoning and the attractive grace of his delivery.

But it was by no means only in lectures that Dumas has sown broadcast the seeds of chemical science. He was, in fact, the first in France to adopt that efficient system of laboratory teaching so happily inaugurated by Liebig, which has ever since been a prominent feature of the German Universities. The laboratory which he had established in the École Polytechnique, though well adapted for an experimental inquirer working along with his assistant, was altogether unfit for the reception of a number of pupils. That he might be able to associate

with experimental students, he founded, as early as 1832, a laboratory of research at his own expense.

Almost immediately after the Revolution of February new labours of the most diverse kind began to encroach upon Dumas' scientific work. The political and social upheaval of 1848, shaking, as it did, the stability of all French institutions, turned into political and administrative courses many men of mark whose energies had been hitherto exclusively devoted to the service of science. It would have been strange, indeed, had not the want been felt of securing Dumas' well-tried powers for the public affairs of the country. Election to the National Legislative Assembly, appointment as Minister of Agriculture and Commerce, admission to the Senate, President of the Municipal Council of Paris, and nomination as Master of the Mint of France, are the steps by which he rapidly rose in his new career.

With the fall of the second Empire the political and administrative career of Dumas came to an abrupt termination. The Senate had ceased to exist, and in the stormy days which followed, the Municipal Council had naturally changed its composition; and even in the Mint, where his rich experience and his rare talent of organisation might have been still of such use in the public service, the man who played so conspicuous a part under the Imperial Government had to vacate his place.

Having thus withdrawn from his official positions, Dumas found himself at the age of seventy in the possession of *otium cum dignitate;* but he never allowed himself to enjoy it in any other than the Ciceronian acceptation of the words. After his retirement from political and municipal life Dumas once more exclusively belonged to science. There was no chemical aspiration which he was not anxious to assist, no problem in the domain of chemistry, physics, or physiology to the solution of which he was not happy and proud to contribute, no scientific movement of any kind to the furtherance of which he was not willing to open the treasury of his matured experience or to lend at least the prestige of his name. But he was never more happy than when, in furthering science, he was at the same time able to serve the material wants and to promote the well-being of his fellow-citizens.

That to scientific services continued for upwards of half a century science should have accorded with unstinted hand a rich share even of her external marks of honour was but to be expected; no Academy, no learned Society but has deemed it an honour to inscribe the name of Dumas on its register. A member of the French Institute at the early age of thirty-two, he has in due time reaped the full harvest of distinctions in store for successful cultivators of science. He became a correspondent of the Berlin Academy of Science in 1834, and a Foreign Associate of that body in 1880; he was elected a Foreign Member of the Royal Society of London in 1840. He was an hono-

rary member of the English, French, and German Chemical Societies. These associations, the second of which originated in Dumas' laboratory, elected him as a matter of course immediately after their institution. In 1843 the Royal Society awarded to him the much-coveted Copley Medal. That he was the first to obtain the Faraday Medal in the gift of the Chemical Society of London, has been already mentioned. Dumas was a Knight of the Prussian Order *pour le Mérite*, the highest scientific honour Germany can bestow, and it may further be added that he received the Grand Cross of the Legion of Honour, and was a Knight of a goodly number of other Orders in Christendom.

In the autumn of 1883 Dumas' health, which up to that time had been unimpaired, began to fail. By the advice of his physicians he passed the winter in the south of France. He died at Cannes on 11th of April, 1884. A. W. H.

[For further information the reader is referred to a sketch of Dumas' life and labours, by Professor A. W. Hofmann, of which the above notice is an abstract. See series of Scientific Worthies ("Nature," 1880, Feb. 6).]

A short account of the life of Dr. TODHUNTER, founded on personal knowledge, and information derived from papers and letters and notes communicated by his relations, has recently been published by his intimate friend, Professor Mayor. From this pamphlet we learn that Dr. Todhunter was born in 1820, and that he was the second of four sons of a Congregationalist Minister at Rye. We are also told that as a child he was unusually backward, and gave no promise of his future eminence. If this be correct, it is a fact from which many boys may draw some encouragement. Passing over his boyhood we find him an assistant-master in a school at Peckham, and at the same time attending the evening classes at University College, and among others the lectures of De Morgan. Here he seems to have come under the fascination which so many of the pupils of that great teacher experienced. We are told that his admiration for that mathematician was unbounded. Need we wonder that the influence of that teaching is seen in so many of the books he afterwards wrote? In 1839 he matriculated in the University of London, obtaining the exhibition for mathematics, and in 1842 he carried off the scholarship at B.A. Finally in 1847 he gained the gold medal at M.A., which was then the highest honour to be obtained in that University. Having thus acquired the means of coming to Cambridge he entered at St. John's College, and took his degree in 1848. That he would be Senior Wrangler and take the first Smith's Prize was never doubtful. Coming up rather older than men usually do, and having brilliant talents, he found himself so much in advance of his year that he was

able to devote a great part of his attention to other than the usual studies. He once told the writer of this notice how as an undergraduate he read Electricity to fill up his time, though the subject did not then enter into the Tripos Examination List.

In the same year in which he took his degree he gained the Burney Prize. According to the regulations, this prize is to be awarded to a graduate of the University who is not of more than three years' standing from admission to his first degree, and who shall produce the best English Essay "on some Moral or Metaphysical subject, on the Existence, Nature, and Attributes of God, or on the Truth and Evidence of the Christian Religion." His essay was printed in 1849 under the title "The Doctrine of a Divine Providence is inseparable from the belief in the existence of an absolutely perfect Creator." These headings are mentioned here to show how widely he was accustomed to spread his studies.

Soon after his degree he established himself in his college as a mathematical tutor, and then acquired a great reputation for his skill as a teacher. Afterwards when new arrangements were made at St. John's College he was appointed Principal Lecturer, and was expected to devote his teaching powers chiefly to the service of his college. Soon after this he vacated his fellowship by marriage, and following the rules of the college he retired from his position at the head of the staff of lecturers. He continued, however, to lecture for some years, but gradually he turned his attention more and more to the work of writing books. Finally he gave up all share in the tuition of his college, and devoted himself to those labours by the results of which he is best known.

Dr. Todhunter also spent much of his time as an Examiner. He was Examiner for the University of London for the five years ending 1869. He examined for the Indian Civil Service Commissioners more than once. He was Senior Moderator in 1865 and Senior Examiner in the following year, but though asked several times to examine again he always declined, declaring that the work was so onerous that it took up too much of his time. This is an opinion held by many other distinguished mathematicians who have examined once. At the same time this circumstance illustrates the care and patience usually given to the preparation of examination papers. He also examined for the Smith's Prize. But the Mathematical Tripos did not alone obtain his attention, he also examined for the Moral Sciences Tripos in the three years 1863, 1864, and 1865.

He was a member of many learned Societies. He became a Fellow of the Royal Society in 1862, and served on the Council during the years 1871–1873. He was elected a member of the Mathematical Society of London in 1865, the first year of its existence. He was also a member of the Royal Astronomical Society.

Some time after his marriage he was elected to be an Honorary Fellow of his college. This was a distinction which he evidently prized so much that he sometimes placed this title by itself after his name, joined solely to the letters M.A. or F.R.S. Later on in his life he was chosen as an Elector to three University Professorships, viz., the Knightbridge Professorship of Moral Philosophy, the Plumian Professorship of Astronomy and Experimental Philosophy, and the Professorship of Mental Philosophy and Logic. Lastly, when the University of Cambridge established its new degree of Doctor of Science, restricted to those who have made original contributions to the advancement of science or learning, he was one of those whose application was granted within the first few months.

The great work of Dr. Todhunter's life lies in the part he has taken in the education of this generation. There are few of us who have not studied some of his books. Many have made their first acquaintance with mathematics through his aid; and not their first acquaintance only, for his books conduct the student through a vast range of mathematical learning. Writing to his wife in 1878, when he was Examiner for the Indian Civil Service, he says: "There is a library of mathematical books provided by the Civil Service Commissioners for the use of the Examiners. It consists of fourteen volumes, ten of which are by myself. Thus you see I am able to do much of that labour which Matthew Arnold thinks distasteful, namely, that of perusing your own books."

A detailed account of the numerous educational books he has written would be too long for so slight a sketch of his life as the present. A simple list of these books is a history of the labours of his life, as the dates run on we see his time filled up with correcting one edition after another.

In writing, his first care was to be accurate. He once told the writer of this notice that, with the assistance of two of his pupils in correcting the press, the first misprint in his "Integral Calculus" did not occur till past the seventieth page. It might have been thought that he would have stereotyped his elementary books, but this was not done until many editions had been issued. Though at pecuniary loss, and with great labour, he yet preferred to correct edition after edition in hopes of eliminating all errors.

In constructing his books, he seems to have discovered that, for the teaching of boys, novelties would be out of place. What was wanted in any subject was a short and accurate account of the things then known. The object was to put the reader as quickly as possible in possession of all the knowledge which was most likely to be useful to him afterwards. Accordingly he gives in his books a clear statement of the well-known principles of each subject, arranged in a logical order. Each step in the argument is explained at length in clear

English. Nothing is assumed but what a reader should know.' Every page makes it evident how thoroughly he was keeping in mind that he was writing for beginners.

Whatever his own ideas were, his books were certainly a great success. His "Euclid" and his "Elementary Algebra" have in twenty years run through fifteen or sixteen editions. They were appreciated by the schoolmasters, and by those who had to teach these subjects. With their recommendations, the sale has grown into something enormous. His more advanced text-books being addressed to a more limited circle of readers could not be expected to run through so many editions; yet we find his "Differential Calculus" reaching its ninth and his "Integral Calculus" a seventh edition.

His reputation in future time will undoubtedly rest on his histories, for the fashion of elementary books will pass away, and a new generation will like a new arrangement of old things. The most important of these are (1) "A History of the progress of the Calculus of Variations during the nineteenth century:" 1861; (2) "A History of the Mathematical Theory of Probability from the time of Pascal to that of Laplace:" 1865; (3) "A History of the Mathematical Theories of Attraction and the Figure of the Earth from the time of Newton to that of Laplace:" 1873. The first of these is a continuation of Woodhouse's history of the Calculus of Variations from its origin until the close of the eighteenth century; and it has been stated that it was his admiration for this work that led him to write this history.

These books appear to the writer of this notice to be of great importance. It is a great boon to the student to have a short and clear account of what has been already done, and what remains to be accomplished in any subject. Though the third of these histories extends over two volumes of nearly five hundred pages each, yet these are not too much for so great a subject.

It is unnecessary to give a particular account of these histories, as they have now been some time before the public. But we would call the attention of those who have not yet read them to their extreme interest. As we read one of them, it seems as if a new light were thrown on the subject. The difficulties of each investigator are put before us; we see how the subject advances, each discoverer adding a little, until step by step we arrive at our present state of knowledge. We see here sketched out before us the gradual growth of those modern methods which we now find so ready to our hands. Thus, in one place, Dr. Todhunter points out the first appearance of those confocal shells which play so important a part in modern works of attraction. These appear in a memoir of Maclaurin's, who introduces them in a remarkable manner without appearing to realise their

importance. In another place, we find a sketch in eight pages of a memoir of Legendre's which Dr. Todhunter considers to be the foundation of all that Laplace added in the theories of attraction and the figure of the earth to the works of Maclaurin and Clairaut. As we read the sketch, we see the first beginning of Laplace's coefficients and a recognition of the importance of the potential. This was the commencement of a new era in mathematical physics. In a third place, the history shows us how D'Alembert, trying to find the attraction of an ellipsoid, makes it depend on a single definite integral. This result, Dr. Todhunter reminds us, is the point at which modern investigations have finally arrived. But D'Alembert, after effecting this, strangely rejects his result as inadmissible. "In his process," says Dr. Todhunter, "there is nothing wrong in principle, but he has omitted a bracket which renders his result slightly inaccurate. He gives some invalid argument against his method. Thus D'Alembert deliberately rejects one of the most important formulæ of the subject, which in fact quite supersedes a large part of his memoir. This is perhaps the strangest of all his strange mistakes." A little further on in the history, we read how Laplace values and appropriates the treasure which D'Alembert deliberately threw away.

In 1869, the subject prescribed for the Adams' Prize was "A determination of the circumstances under which Discontinuity of any kind presents itself in the solution of a problem of Maximum or Minimum in the Calculus of Variations." The proposal of this subject seems to have arisen from a controversy which had been carried on in the "Philosophical Magazine" a few years previously. In this controversy Dr. Todhunter had taken part, and when the subject was proposed for the essay he was anxious that his own view should prevail. This view is given in the opening sentences of his essay:—"We shall find that, speaking generally, discontinuity is introduced by virtue of some restriction which we impose, either explicitly or implicitly, in the statement of the problems which we propose to solve." This thesis he supports by considering in turn the usual applications of the calculus, and pointing out where he considers the discontinuities which occur to have been introduced into the conditions of the problem.. This he successfully proves in many instances. In some cases, the want of a distinct test of what discontinuity is, somewhat obscures the argument. His essay was rewarded with the prize. It is published under the title "Researches in the Calculus of Variations."

In the midst of so busy a life, Dr. Todhunter could yet find time to write for others. The second edition of Boole's "Differential Equations" was published under his care; and, what is more, he undertook the labour of arranging and editing the supplementary

volume. This task was undertaken from friendship to the late Professor Boole. The difficulty of preparing unfinished papers for the press is obvious; and it is not surprising that, as he once mentioned to the writer, it should have cost him some months of hard work.

Dr. Todhunter has left a treatise on Elasticity, which was very nearly finished. The time and labour he spent over this work injured his eyesight, and probably led to his final illness. These MSS. had been sent to Professor Cayley to report on; and we learn from Professor Mayor's pamphlet that the investigation shows that they are of the same class as the history of the Theory of Attraction, and seem fairly complete.

Another result of the labours of his latter years is a treatise on Arithmetic. Such a work when perfected would have smoothed the way for the young beginner over many difficulties. It is greatly to be regretted that he did not undertake it sooner.

In the summer of 1880 Dr. Todhunter first began to suffer from his eyesight, and from that date he gradually and slowly became weaker. But it was not until September, 1883, when he was at Hunstanton, that the worst symptoms came on. He then partially lost by paralysis the use of the right arm, and, though he afterwards recovered from this, he was left much weaker. In January of the next year he had another attack, and on the 1st of March, 1884, he died at his own residence in Cambridge, surrounded by his dearest relatives. It was a fit ending to an honourable and respected life spent in the advancement of that science which he loved so well.

E. J. R.

April, 1884.

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ERRATA.

Mr. Blanford, "On Connexion of Himalaya Snowfall," &c., No. 232:—

Page 5. 9th line from bottom, for "J. A. Hill" read S. A. Hill.
" 8. Bottom line of table, column 1875, for "14" read 114.
" 10. Line 22, for "Kailong" read Kailang.
" 11. Line 21, for "Dias" read Dras.
" " 3rd line from bottom, for "Taini-Tel" read Naini-Tal.
" 13. Line 1, for "Chakatra" read Chakrata.

Dr. Schuster.—Bakerian Lecture:—

Page 331. 3rd line from bottom, for "uniform force" read uniform magnetic force.

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